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ORIGINAL ARTICLES.

I.—On *Schlotheimia Greenoughi*, J. Sowerby, sp.

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(PLATE IV.)

While working at the Lower Liassic Ammonites of the Sowerby Collection, preserved in the British Museum (Nat. Hist.), the author happened to come upon one of the paratypes of *Ammonites Greenoughi*, J. Sowerby, which could be recognized at once as being allied to *Schlotheimia Charmassei*, d'Orbigny, sp. On going into the matter in greater detail it was found that the suspicions of some Ammonite workers regarding the misinterpretation of the species by most previous authors were, indeed, well founded. Quenstedt,[1] for example, had stated that Wright, having found Sowerby's type very much disfigured by decomposition of pyrites, substituted for it a gigantic specimen of a diameter of 440 mm., "which, however, looks different again." Pompeckj[2] again stated that the gigantic specimen drawn by Wright, as well as his description defined the species in a not very precise manner, and that, therefore, it was not certain that the specimen agreed with Sowerby's original.

It will be shown in the following pages that whereas *Amaltheus Greenoughi* (Sowerby), Wright, had, by most recent writers, been regarded as an *Oxynoticeras*[3] from the *oxynotum* zone, Sowerby's form is a *Schlotheimia*, and appears to be restricted to the *Bucklandi* beds, or, more correctly, to the *rotiforme* subzone[4] only. The matter seemed of sufficient importance to be recorded without delay.

There are now in the Sowerby Collection in the British Museum two specimens bearing the original Sowerby label, "*Ammonites*

[1] F. A. v. Quenstedt, *Die Ammoniten des Schwäb. Jura*, vol. i, p. 297, 1885.

[2] J. F. Pompeckj, "Notes sur les Oxynoticeras, etc.": Comm. Serv. Geol. Port., vol. vi, fasc. 2, p. 264, 1906.

[3] Neumayr in 1875 ("Die Ammon. der Kreide, etc.": Zeitschr. Deutsch. Geol. Ges., vol. xxvii, p. 886) first put *A. Greenoughi*, Sow. (Hauer) into the genus *Amaltheus*, and in 1878 ("Über unvermittelt auftretende Cephalop.": Jahrb. k. k. Reichsanst., vol. xxviii, p. 61) he included it in the group *Fissilobati* of that genus, together with a heterogeneous mixture of other Ammonites. Wright, in 1882, followed Neumayr, but Tate & Blake (in *Yorkshire Lias*, 1876, p. 296) referred the form to the genus *Phylloceras*; and in H. B. Woodward (*Jurassic Rocks of Britain*, vol. iii, p. 336) both *Amaltheus* and *Phylloceras* are given. S. S. Buckman (in L. Richardson, *Geology of Cheltenham*, 1904, p. 212), on the other hand, assigned the form to the genus *Agassiceras* [*Agassizoceras*].

[4] i.e. Lower *Bucklandi* zone; see S. S. Buckman, *Yorkshire Type Ammonites*, vol. i, p. xvi, 1910.

Greenoughi, M.C. T. 132," as well as a very large specimen of a diameter of 520 mm., the history of which is unknown. Although the catalogue handed to the Museum with the collection records, in J. de C. Sowerby's handwriting, only one example of *A. Greenoughi*, it is probable that the Sowerby Collection originally included three specimens, namely: (A) the figured specimen (holotype) which, according to Wright,[1] is decomposed and cannot now be traced; (B) the smaller of the specimens afore-mentioned (B.M. Geol. Dept., No. C 17640); (C) a larger specimen of 440 mm. diameter (B.M., No. C 17641).

The smaller of these two paratypes, i.e. specimen B, best illustrates the species, and is, therefore, now taken as the lectotype (see Plate IV). It is suggested that Wright did not know of the existence of this specimen when he described the form. It measures about 230 mm. in diameter, but has two-fifths of the outer whorl missing and also the innermost whorls. The specimen consists of a cast in iron-pyrites filled loosely with calcite crystals in large scalenohedra. Its body-chamber portion, marked "Part of the large specimen which is in fragments", now measures only about one-quarter of a whorl, but, to judge by the umbilical junction preserved on the previous whorl, originally consisted of at least three-quarters of a whorl. On the accompanying Plate IV this body-chamber fragment is omitted, but it would reach from *a* (the end of the septate stage) to *b*. The complete Ammonite then measured some 320 mm. in diameter. This body-chamber portion consists of a bluish-grey limestone, weathering to a softer and lighter-coloured stone, typical of Somerset and especially the Bath–Keynsham district.

The specimen cannot be the entirely septate Ammonite figured by Sowerby,[2] for it is of quite a different appearance. Even if it had been entire and in good condition at the time of preparation of Sowerby's figure, the short distance of its conspicuous last suture[3] from the end of the costate stage would almost certainly have been correctly indicated by Sowerby. The dimensions agree, however, as the following figures show, testifying to the probable accuracy of Sowerby's delineation.

	Diameter.	Height.	Thickness.	Umbilicus.
Sowerby's figure .	113 mm.	47 per cent	?24 per cent	21 per cent
Specimen B at .	140 ,,	48 ,,	?26 ,,	21 ,,
,, at .	203 ,,	47 ,,	?23 ,,	21 ,,

Now Wright states that the specimen he studied had a diameter of 440 mm. But this diameter agrees with the second specimen (C) of the Sowerby Collection, whereas the one that Wright[4] figured

[1] T. Wright, *Lias Ammonites* (Mon. Pal. Soc.), 1882, p. 384.

[2] *Mineral Conchology*, vol. ii, p. 71, pl. 132, 1816.

[3] This distance, in the large, comparable specimens, amounts to about one whole whorl, and it is probable, therefore, that Sowerby's figure is reduced to, perhaps, as much as one-third of the original diameter, especially since he states that specimens vary in size from 12 to 18 inches in diameter. The suture-line itself has the lateral saddle higher and larger than the external saddle, and is apparently of a *Schlotheimia* pattern. Only the last ones at about 200 mm. diameter are visible (at *a* in the figure), but they are, unfortunately, not clearly traceable.

[4] T. Wright, op. cit., pl. xliv.

(B.M. No. 82363) has a diameter of 520 mm. Wright does not mention that he had two specimens; but his figure evidently is a composite one, the obscure costæ of the outer whorl showing on the former specimen only, though in a less distinct manner. Moreover, whereas his figured specimen has the locality "Somerset" attached to it, Wright says that no locality was recorded, which again is the case with C, Sowerby's specimen of 440 mm. diameter. Wright thought that it probably came from Lyme Regis, "from the petrological character of the matrix in which it was embedded," but this is doubtful, as also is Morris's[1] mention of the species from Lyme thirty-nine years earlier, and the record of *A. Greenoughi* from the [*oxynotum* zone ? of the] neighbourhood of Cheltenham both by J. Buckman[2] and by S. S. Buckman.[3]

Sowerby's specimen C of 440 mm. diameter has the following dimensions:—

Height,	42	per cent of the diameter.
Thickness, ?	24	,, ,,
Umbilicus,	29	,, ,,

On comparing these dimensions with those of the lectotype (B) it will be seen that, whereas the height of the whorl has diminished from 47 to 42 per cent, the umbilicus has widened considerably (from 21 to 29 per cent). It appears probable that this is the normal development of the form, i.e. that it is a case of latumbilication arising from excentric coiling. And specimen C, indeed, clearly exhibits this excentrumbilication, though not the specimen figured by Wright. The latter has at a diameter of 520 mm. the following dimensions:—

Height,	42	per cent of the diameter,
Thickness, ?	24	,, ,,
Umbilicus,	30	,, ,,

whereas his (reduced) figure shows—

Height,	41	per cent of the diameter,	
Umbilicus,	31	,, ,,	(34 per cent in the text).

These dimensions agree well with those of the large specimen (C) mentioned above, and also show that Wright's delineation was fairly accurate so far as the dimensions are concerned, although in the absence of a sectional view the shading suggests thicker whorls and the obscure costæ on the body-chamber are too pronounced. Important differences appear, however, when we compare the costation of the inner whorls of these two large specimens with that of the lectotype. The latter has, counting backwards from the last rib at a diameter of about 145 mm., twenty and fifteen primary costæ respectively for these two whorls. They give rise to about two or three secondaries each, and these are continuous across the periphery at a diameter of 90 mm. when they weaken and finally, at 200 mm. diameter, almost entirely disappear.

[1] John Morris, *Catalogue of British Fossils*, 1843, p. 173.
[2] In Murchison, *Geology of Cheltenham*, 2nd issue, 1845, p. 89.
[3] In Richardson, *Geology of Cheltenham*, 1904, p. 212 (as *Agassiceras* [*Agassizoceras*] *Greenoughi*).

The inner whorls of the two larger specimens could not be exposed, and comparison therefore is difficult. But whereas Sowerby's specimen (C) has twenty-eight and nineteen costæ respectively on the two last wholly costate (inner) whorls, the specimen figured by Wright has twenty and sixteen respectively, again counting backwards from the last distinct rib shown in the umbilicus. The latter number is the same as in the lectotype; but Wright's specimen differs both from the latter and from the more closely costate specimen (C) in having more regularly (not excentrically) coiled inner whorls which are coarser and rounder. The outer whorl of Wright's specimen, on the other hand, is flatter and smooth.

Since neither of the large specimens therefore agrees with the lectotype, it seems advisable to refer to these forms only as "*Schlotheimia* spp. ex aff. *Greenoughi* (Sowerby)", until duplicate specimens enable us to study their inner whorls and to define their (possibly specific) differences from the lectotype more definitely. What Wright[1] says about the resemblance of the young to *Oxynoticeras guibalianum* (d'Orbigny) and the presence of a keel in specimens of from 6 to 8 inches in diameter is, of course, erroneous. Sowerby had stated that the "undulations are continued and rather strongest over the rounding back", and though in his figure that author did not give quite the correct course of the radial line, the above remark obviously did not point to connexion with *Oxynoticeras*. Wright here followed previous authors, especially von Hauer, whose *A. Greenoughi*,[2] like that figured by Parona,[3] has now been included in Fucini's *Oxynoticeras Haueri*.[4] Sowerby's Ammonite, unfortunately, has been misinterpreted by Continental writers since the time of von Buch. That author figured as *A. Greenoughii*[5] an Ammonite that Quenstedt[6] thought might be the same as *A. Tessonianus* of d'Orbigny from the iron-oolite δ (i.e. *witchellia* zone of the Inferior Oolite), and Giebel[7] mentions that Graf Münster records it from the Oxford Clay. Studer[8] has it from the Upper Lias (Toarcian), and Ooster's *A. Greenoughi*[9] again is, according to Hug,[10] an *Oxynoticeras* of *guibalianum* type. Dumortier[11] also probably misinterpreted the species when he recorded it from the *oxynotum* zone.

[1] T. Wright, *Lias Ammonites*, p. 385.

[2] F. v. Hauer, "Über die Cephalop. aus dem Lias der Nord-Östlichen Alpen": Denkschr. Akad. Wiss. Wien. Math.-Naturwiss. Klasse, vol. xi, p. 46, pl. xii, figs. 2–4, 1856.

[3] C. F. Parona, "Ammon. Lias infer. Saltrio": Mem. Soc. Pal. Suisse, vol. xxiii, p. 18, pl. i, fig. 2, 1896.

[4] A. Fucini, "Cefal. Lias. del Mte. di Cetona," part i: Pal. Ital., vol. vii, pp. 8–9, 1901.

[5] L. v. Buch, *Explication de trois Planches d'Ammonites*, 1830, pl. i, figs. 2*a–c*.

[6] F. A. v. Quenstedt, *Handbuch der Petrefaktenkunde*, 1852, p. 364.

[7] C. G. Giebel, *Fauna der Vorwelt*, vol. iii, p. 554, 1852.

[8] B. Studer, *Geologie der Schweiz*, vol. ii, p. 36, 1853.

[9] W.-A. Ooster, *Cat. Ceph. Foss.*, vol. vi, p. 45, pl. xvi, figs. 1, 2, 1863.

[10] O. Hug, "Beitr. Kenntn. Lias & Dogger Amm. Freiburger Alpen," II: Abh. Soc. Pal. Suisse, vol., xxvi, p. 6, 1899.

[11] E. Dumortier, *Etudes Paléont. Dépots Jurass. Bassin du Rhone*," vol. ii, p. 148, 1867.

De la Beche's[1] mention of the form (he alters the name to *A. Greenovii*) from the Lower Oolite of Bayeux, and a diagrammatic figure of it published by T. Brown,[2] were equally unfortunate, and Blake[3] apparently confused Sowerby's Ammonite with *oxynoticerates* of *Salisburgense* type, since he records two specimens of it from the *oxynotum* zone of Yorkshire, though he says that one specimen, doubtfully identified with it, occurred in the *Bucklandi* beds near Redcar. But it is chiefly on Wright's authority that more recent writers, including Hyatt[4] and Pompeckj,[5] put *A. Greenoughi* with or near *Oxynoticeras guibalianum*, d'Orbigny, sp., and into the *oxynotum* zone.[6] Pompeckj, who, as has already been pointed out, is not satisfied about Wright's interpretation of the species, mentions that *O. Greenoughi* is also quoted from the *Bucklandi* zone of England, "or more correctly the subzone of *Ariet. semicostatum*=subzone of *Ariet. geometricus* or of *Pentacrinus tuberculatus*." In fact, in Somerset (at Bath, Keynsham, etc.), whence probably all the known specimens came and whence the species is recorded by Lonsdale[7] and Morris,[8] it is associated with *Vermiceras* and *Coroniceras*, and therefore belongs to the *rotiforme* or Lower *Bucklandi* zone and not to the *geometricum* or Upper *Bucklandi* zone.

With regard to the other forms of *Schlotheimia* that occur in the *Bucklandi* zone, the dimensions are given below of three large examples quoted by Pompeckj[9]—

	Diameter. mm.	Height. per cent	Thickness. per cent	Umbilicus. per cent
Schlotheimia Charmassei, d'Orbigny, Pompeckj (= *A. angulatus compressus*, Quenstedt, ii, 2, 1883) .	149	46	24	24
Schl. intermedia, Pompeckj (= *A. angulatus intermedius gigas*, Quenst., pl. iv, fig. 1, 1883) .	600	38	18	32
Schl. d'Orbignyana (Hyatt), Pompeckj (= *A. angulatus compressus gigas*, Quenst., pl. iv, fig. 2, 1883) . .	420	48	18	21

[1] H. de la Beche, "On the Geology of Part of France": Trans. Geol. Soc., ser. II, vol. i, p. 80, 1822.

[2] T. Brown, *Illus. Foss. Conch.*, etc., 1849, p. 12, pl. ix, figs. 7, 8.

[3] In R. Tate & J. F. Blake, *Yorkshire Lias*, 1876, p. 296.

[4] A. Hyatt, "Genesis of the Arietidæ": Smithsonian Contributions to Knowledge, 1889, p. 218.

[5] J. F. Pompeckj, op. cit., p. 264.

[6] E. Böse, however, had pointed out in 1894 ("Fleckenmergel": Zeitschr. Deutsch. Geol. Ges., vol. xlvi, p. 747) that *A. guibalianus*, d'Orbigny, sp., and *A. Greenoughi*, J. Sowerby, could not be united as they were by Hyatt, since the ribbing was quite different "if one could judge at all from the bad original figure". We have seen already that Sowerby's figure was for his time very good, but authors, unfortunately, do not seem to have consulted the text. On p. 70 Sowerby also made the important remark (which apparently had quite escaped attention) that *A. Conybeari* and *A. Greenoughi* were generally companions in the same stratum, and were occasionally impressed with each other's type.

[7] W. Lonsdale, *Trans. Geol. Soc.*, ser. II, vol. iii, p. 272, 1832.

[8] J. Morris, *Cat. Brit. Foss.*, 1843, p. 173.

[9] J. F. Pompeckj, *Beiträge z. e. Revision der Ammon. Schwäb. Jura*, pt. i, pp. 81–3, 1893.

The first is clearly of the type of *Schlotheimia Greenoughi*, though just a trifle too evolute, since its umbilicus measures 24 per cent, instead of 21 per cent at the same diameter. It is also too densely costate, as is d'Orbigny's original figure of *A. Charmassei*[1] (especially on the inner whorls) and Wright's '*Ægoceras*' *Charmassei*.[2] The latter also has a sulcate periphery to a larger diameter than the others, and Dumortier's *A. Charmassei*[3] is distinguished by its flexicostation, as well as by the closeness of its ornament.

S. intermedia, Pompeckj, greatly resembles the two large forms that are here considered to be allied to *S. Greenoughi*, but may be a little thinner, though the exact thickness of the latter cannot be determined owing to the corrosion of one side of the specimens. The costation differs in being a little too close.

S. d'Orbignyana (Hyatt), Pompeckj, has at a diameter of 420 mm. the dimensions of the considerably smaller example of *S. Greenoughi*, i.e. it remains involute; but its costation also is too close and the periphery too sharp.

Finally, as an illustration of the excentric coiling of some forms of *Schlotheimia*, it may be mentioned that in *S. depressa* (Wähner), Pompeckj[4] (= *A. angulatus thalassicus*, Quenstedt, 1883, pl. ii, fig. 4 only), the inclusion decreases from 41 to 28 per cent in less than a whorl, whereas in *S. thalassica*, Quenstedt, sp. (= *A. angulatus thalassicus*, Quenstedt, 1883, pl. ii, fig. 5), the umbilicus, owing to the rapid increase in height of the last whorl, decreases from 50 to 37 per cent at diameters of 32 and 100 mm. respectively.

In conclusion, I should like to express my best thanks to Dr. A. Smith Woodward, through whose kind offices I was permitted to work on the rich store of Ammonites in the British Museum, and to Mr. G. C. Crick for placing his extensive knowledge of the collection at my disposal.

II.—Radio-activity and the Earth's Thermal History.

By Arthur Holmes, A.R.C.S., D.I.C., B.Sc., F.G.S.

PART II.

Radio-activity and the Earth as a Cooling Body.[5]

7. Other Sources of Terrestrial Heat.

So far, the source of the heat lost from the earth has been assigned only to the disintegration of the radio-active elements. Besides this, heat is liberated near the surface by the processes of weathering. The decomposition of one gram of average rock is accompanied by the generation of about 120 calories. Owing to its extreme slowness, this

[1] A. d'Orbigny, "Paléont. Franç. Terr. Jurass. Ceph.," pl. xci, figs. 3, 4, *non* 1–2, *nec* pl. xcii (see S. S. Buckman, "Some Lias Amm.": Proc. Cotteswold Club, vol. xv, pt. iii, p. 239, 1906).

[2] T. Wright, op. cit., 1880, pl. xx.

[3] E. Dumortier, op. cit., 1867, p. 29, pl. xvii, fig. 1.

[4] See J. F. Pompeckj, op. cit., p. 78, and F. Wähner, "Beitr. Kenntn. tieferen Zonen d. Unt. Lias i. d. Nö. Alpen," part iii: Beitr. Pal. Österr.-Ung., vol. iv, p. 164, 1886.

[5] Part I, "The Concentration of the Radio-active Elements in the Earth's Crust," appeared in the February Number, pp. 60–71.

source of heat generation is of practically no importance except locally, where for a time it may succeed in appreciably steepening the temperature gradient. The heat liberated by weathering is probably balanced by the absorption of heat at greater depths, which is involved in the processes of metamorphism. Of much greater importance is the question whether the earth can still be regarded as possessing an ancient heritage of heat dependent on its origin.

According to the Laplacian hypothesis the earth began as a hot gaseous spheroid, and its subsequent history has been largely dependent on the gradual cooling from that initial state. The Meteoric hypothesis of Lockyer leads to an earth having a similar thermal history. In each of these cases original heat is regarded as by far the most important source of the earth's thermal energy. Recently, however, both of these hypotheses have been discredited on dynamical and other grounds, and Professor Chamberlin has introduced his Planetesimal hypothesis as an alternative which successfully overcomes the difficulties encountered by the older views. According to Chamberlin [1] (writing in 1905) there are four main sources of heat arising from the mode of origin postulated by the Planetesimal hypothesis. (1) The original earth nucleus about which the planetesimals gathered may have been hot, the heat being partly original, partly the result of condensation. (2) By the infalling of planetesimals heat would be produced. If the infalling were sufficiently rapid, the heat newly generated would be retained and the surface might be kept in a state of fusion. It is considered, however, by Chamberlin that in the later stages the rate of accretion became so slow that most of the heat generated by impact was radiated away. (3) The chief source of heat is assigned to mechanical compression in the interior of the growing earth. (4) Another effect of compression would be a molecular re-arrangement by which denser compounds would be produced. It is thought probable that this re-combination would be attended by the liberation of thermal energy.

Dr. A. C. Lunn [2] has discussed mathematically the thermal condition of the earth on the basis of the Planetesimal hypothesis. He takes into account only heat derived from gravitational energy, remarking that the heat obtained from other sources might be relatively important, and that if it were of sufficient magnitude it would completely alter the thermal process.[3] According to the assumptions made, which are admitted to be uncertain, Lunn finds the temperature gradient near the surface to be only 330° C. in 200 miles (0·00001° C. per cm.). The actual present gradient is about 0·00032° C. per cm. Such a result, obtained before the geological significance of radio-activity was realized, appears to be in complete harmony with the fact that radio-activity seems to leave little room for any appreciable temperature gradient due to an original store of heat. Writing in 1911 Chamberlin pointed out this co-operation of planetesimal and radio-active agencies,[4] and he also indicated the

[1] Chamberlin & Salisbury, *Text Book of Geology*, vol. ii, p. 100, 1906.
[2] Carnegie Inst., Washington, Pub. No. 107, 1909, p. 169.
[3] Lunn, loc. cit., p. 230.
[4] *Journ. Geol.*, p. 673, 1911.

additional difficulty of maintaining belief in a molten earth which the new source of energy had introduced. This difficulty, however, which has been very generally realized, is really one of time. If the earth has cooled down at all, the effect of radio-activity will have been to slow down the rate of secular cooling. What the presence of radio-activity does prove in such a case is that the earth must be very much older than the twenty or forty million years allowed by Kelvin for the process of cooling from a molten state to present conditions.

8. The Age of the Earth.

A very simple method will serve to show approximately the effect of radio-activity on the normal rate of cooling, and on the age of the earth to be deduced from it. The temperature gradient $d\theta/dx$ is inversely proportional to the square root of the time of cooling, t, which is, on this view, the age of the earth (i.e. $d\theta/dx \propto 1/\sqrt{t}$). If five-sixths of the present temperature gradient be due to radio-activity, then only one-sixth can be attributed to cooling. The above formula indicates that if the gradient $d\theta/dx$ is reduced to one-sixth, then the time of cooling, t, must be thirty-six times as great. The effect of radio-activity is thus to raise Kelvin's limits from twenty and forty million years to 720 and 1,440 million years. A more rigid mathematical treatment shows that radio-activity is even more powerful in slowing down the rate of cooling than appears from these figures, and it becomes clear that if ever the earth was molten at the surface it could not, with the existing distribution of the radio-active elements, have cooled down to its present state in less than 1,600 million years.

It is therefore of interest to know that an independent method of determining geological time—though also based on radio-activity—gives results which for the oldest rocks closely approach this figure. Every uranium-bearing mineral is like a clock ticking out its age in molecules of helium and lead. Helium is generated also during the decay of thorium, but the end-product of thorium is still unrecognized. It cannot be an equivalent of lead, since in thorium minerals this element does not accumulate in geological time.[1] In uranium-bearing minerals, however, lead does accumulate, and the rate of its production is accurately known.[2] In any primary mineral the amount of uranium engaged in the generation of lead, and the amount of lead which has accumulated, factors which can generally be estimated by analysis, suffice to determine the age of the mineral. The geological time scale, as far as at present determined, is given on p. 105.

The oldest rocks in this table, which appear to be the gneissose granites of Mozambique, are probably not more ancient than the oldest rocks in any of the great Archæan areas. In Fenno-Scandia and North America there are rocks of much greater geological antiquity than those for which ages are here given. There is, naturally, great difficulty in obtaining suitable minerals for analysis.

[1] Holmes & Lawson, *Phil. Mag.*, vol. xxviii, p. 823, 1914.

[2] For a full discussion of this method of estimating the age of minerals, see Proc. Roy. Soc., A, vol. lxxxv, p. 248, 1911, and *The Age of the Earth*, ch. x, 1913.

When such material is collected from the oldest rocks of the Archæan shields, it is probable that the ages will be found to approximate to 1500–1600 million years.

For the purposes of this paper the period which has elapsed since the crystallization of the oldest gneissose granites or orthogneisses, a period which may be referred to as the age of the earth, will be taken, independently of thermal considerations, as 1,600 million years. With such an extended period in which to cool, an earth originally molten at or near the surface would now have a temperature gradient so low that it could easily be hidden under the much more imposing gradient due to radio-thermal energy. We must, therefore, examine afresh the evidence for and against an earth originally molten at or near the surface.

Geological Period.	*Age in millions of years.*
Carboniferous, U.S.A.	340
Devonian, Norway	370
Silurian or Ordovician, U.S.A.	430
Pre-Cambrian :—	
Galle pegmatites, Ceylon	800
Unfoliated granites, G. E. Africa	800
Moss complex, Norway	1,000
Ser-Archæan, Sweden	1,100
Arendal complex, Norway	1,250
Algoman (?), Canada	1,200
Intrusive granites, Madagascar	1,250
Intrusive granites, Mozambique	1,100
Gneissose-granites, Mozambique	1,500
Thorianite, Ceylon	1,600 (?)

9. The Thermal Problem of the Original Crust.

As already suggested, Chamberlin holds that the infalling of planetesimals during the later stages of the earth's growth was so comparatively slow that the heat generated was readily radiated away. Indeed, he believes that oceans first became possible at a stage when the earth was much smaller than now, and thus denudation and deposition, both mechanical and chemical, began while the earth was still growing by the capture of planetesimals. Daly raises the objection [1] that if this view is correct the ocean ought to be more salt than it actually is. This criticism is undoubtedly well founded, for the difficulty of reconciling the conflicting estimates of geological time based on salinity and radio-active data respectively becomes still worse if the ocean is considered to be older than the oldest known rocks. Dr. Evans has suggested to me that if the composition of the planetesimals which completed the growth of the earth was at all similar to that of meteorites, then the earliest known sediments, which must have been produced partly from fresh planetesimal material and partly from volcanic rocks and pre-existing sediments, ought to betray the peculiar nature of their source by some peculiarity in their chemical composition. In particular, it is reasonable to expect that they would contain determinable quantities of nickel. This does not seem to be the case, and in fact the earliest

[1] *Igneous Rocks and their Origin*, p. 115, 1914.

sediments do not differ chemically in any essential respect from those of later date. It might, however, be worth while to examine the older sediments or their metamorphic equivalents for a possible nickel content. Such rocks contain manganese, although in meteorites, even in the stony varieties, nickel is always much more abundant than manganese.

The view that the external shell of the earth has not been in a general molten condition depends on the assumption that radiation from the surface was able to keep pace with the heat generated by impact, together with the amount brought to the surface by conduction from the interior and the still greater amount carried by the extrusion of molten tongues of the lighter and, with the aid of volatile fluxes, more fusible materials. As Daly has pointed out,[1] it is very doubtful whether this assumption can be justified.

Another factor suggested by Dr. Evans which must be taken into consideration is the thermal blanketing that would result as soon as an atmosphere was generated. For the primitive atmosphere would be exceedingly rich in carbon-dioxide and water vapour, athermanous substances that would very efficiently absorb thermal radiations and conserve the growing supply of heat within. It is impossible to discuss quantitatively the relation between heat generation and radiation in so complex a system, and it can only be said that the assumption of a solid crust throughout the later stages of growth has to overcome a number of difficulties which would not otherwise be so serious.

There can be no doubt that the mechanism by which granitic and basaltic magmas were differentiated from average planetesimal material and arranged according to their densities would work very much more quickly and effectively if the earth were molten near the surface during the whole period of growth, than if the temperature of fusion were not reached until the freshly fallen planetesimals had been buried to a considerable depth. There is, in fact, clear evidence that fusion temperatures did in the early history of the earth come very near the surface. The great abundance of Archæan granites and orthogneisses throughout the continents proves a widespread molten condition at no very great depth. It may be objected that where the relations between the oldest granites and the oldest sediments are indeterminable, the former are always intrusive. This is, however, just what would be expected, for sedimentation implies submergence, and the rocks constituting the underlying platform of deposition must therefore have been lowered to depths where considerably higher temperatures would be reached. If the temperature gradient were high in these early days, the rise of temperature might easily reach the point of refusion. Moreover, an additional effect of sedimentation would be to cover the underlying platform with a radio-active layer which, as Joly has shown,[2] would substantially raise the basal temperature still further. Thus, granting a sufficiently high temperature gradient in these early times, there is no difficulty in understanding the irruptive contacts of the most ancient igneous

[1] *Igneous Rocks and their Origin*, p. 157, 1914.
[2] *Radio-activity and Geology*, p. 102, 1909.

rocks. Chamberlin believes that the temperature gradient in the outer part of the earth was such as to preserve "equilibrium between solidity and liquefaction according to the conditions present at each depth".[1] The oldest Archæan rocks imply that at the time of their formation the equilibrium between solidity and liquefaction began at no great depth from the surface.

A further criticism made by Daly against the view of a solid exterior during the later stages of growth is based on a comparison of the average densities of the earth and the outer planets and sun. The low densities of the outer planets may be due to high temperature or to the absence of heavy elements. As in the case of the sun, the interpretation of astrophysics is that the temperature is high. If the larger planets are still in a condition of high temperature it seems likely that the earth has passed through an essentially similar stage.

The contraction of the earth evidenced by the phenomena of mountain building is not invoked in this discussion because there are sources of contraction other than cooling, notably mechanical compression, chemical readjustment, and physical change of state. If cooling were admitted as the chief cause of contraction, then the universality of orthogneisses among the earliest igneous rocks and of cleavage, foliation, crumpling, and distortion among the earliest sediments, and the gradual restriction of these features in later geological time to narrow belts on the earth's surface, points at once to more rapid cooling in the outer part of the crust during the earlier of the Pre-Cambrian periods, and therefore to a higher temperature from which to cool. It is doubtful, however, whether this argument is valid. It is certainly true of contraction, but not necessarily true of cooling.

Summing up we may conclude that considerations based on—

(1) the existing salinity of the ocean;
(2) the absence of ancient sediments or schists with significant peculiarities of composition;
(3) the possibility of heat generation being greater than radiation during the later stages of the earth's growth;
(4) the probability of a strongly athermanous primitive atmosphere;
(5) the density stratification of the earth, particularly in its outer shell;
(6) the universality of plutonic igneous rocks and their metamorphic equivalents at no great depth in early Archæan times;
(7) the probable high temperatures of the larger sister planets,

all point to the improbability of a solid exterior during the earth's later stages of growth, while conversely, they all favour the probability that magmatic temperatures existed at or immediately below the surface throughout the period of growth.

10. Mathematical Treatment.

The effect of radio-activity on earth temperatures and the rate of cooling has been ably discussed mathematically by Messrs. L. R. Ingersoll and O. J. Zobel, of the University of Wisconsin, in their recent book on the Mathematical Theory of Heat Conduction (Ginn & Co., 1913). They assume that only a fraction, $1/n$, of the total annual

[1] *Journ. Geol.*, p. 686, 1911.

loss of heat from the earth is supplied from radio-thermal energy, and that the distribution of the radio-elements falls off exponentially in depth. Their final equation (No. 85, p. 95) expresses θ, the temperature at any depth x, and at any time t, in terms of the following data :—

S, the initial temperature at, or just below, the surface.
m, the initial temperature gradient.
k, the thermal conductivity of average rock.
$h^2 = k/c\rho$, the diffusivity of average rock.
c, the specific heat of average rock.
ρ, the density of average rock.
A, the heat production of unit volume of average rock per second.
a, the decrease of heat generation per unit distance in depth.

For the details of the equation and the treatment leading up to it the reader is referred to the original work. Mr. Zobel has very kindly differentiated this equation for me, and much of the mathematical treatment which follows below is due to his help, for which I wish here to express my gratitude. Differentiating equation 85, the temperature gradient for any time t, and at any depth x, is obtained. The temperature gradient $d\theta/dx$ is actually measured at the present time at the surface, so that $x=0$. The equation then becomes

$$d\theta/dx = m + \frac{S}{h\sqrt{\pi t}} + \frac{A}{ak}\left[1 - e^{a^2h^2t} \cdot \frac{2}{\sqrt{\pi}} \int_{ah\sqrt{t}}^{\infty} e^{-\gamma^2}\, \delta\gamma\right]$$

For large values of $ah\sqrt{t}$, the equation may be expressed more simply as

$$d\theta/dx = m + \frac{S}{h\sqrt{\pi t}} + \frac{A}{ak}\left[1 - \frac{1}{ah\sqrt{\pi t}}\right] \qquad (10)$$

The unknowns in this equation are m, S, and a, and obviously two of them must be assumed to be known. It is highly improbable that the initial temperature of the earth was uniform throughout. The work of Barus shows that the temperature of fusion increases with pressure and therefore with depth. Although this law may not hold to any great depth, yet the temperature must also increase with depth as a result of compression. Originally, then, the temperature θ at any depth x would be $\theta = mx + S$.

It is assumed here that:

$$m = 0{\cdot}00005^\circ \text{ C. per cm.}\ ;\ S = 1{,}000^\circ \text{ C.}$$

The known factors in equation 10 are [1]—

$d\theta/dx = 0{\cdot}00038^\circ$ C. per cm.
$t = 1{,}600$ million years $= 5{\cdot}05 \times 10^{16}$ seconds.
$A = 63{\cdot}9 \times 10^{-14}$ calories per second per c.c. of rock.
$k = 0{\cdot}005^\circ$ C. per cm.
$\rho = 2{\cdot}8$.
$C = 0{\cdot}25$.
$h^2 = k/c\rho = 0{\cdot}0071$.
$h = 0{\cdot}084$.

Equation 10 may now be solved for a, which is found to be 4×10^{-7}.

[1] See Part I, GEOL. MAG., Dec. VI, Vol. II, p. 70, 1915.

The first two terms of equation 10 are those which would obtain if no radio-thermal energy were present[1] (i.e. $x = 0$ and $A = 0$). They, therefore, represent the contribution to the temperature gradient furnished by the earth's original thermal condition, and the third term represents that portion of the gradient which is due to radio-activity. The total heat q which issues from unit area of the earth's surface is obtained by multiplying equation 10 throughout by k, since $q = k\, d\theta/\theta x$. We may therefore write—

Total heat flow per unit area due to initial thermal state

$$= mk + Sk/h\sqrt{\pi t}.$$

Total heat flow per unit area due to radio-activity, R,

$$= \frac{A}{a}\left[1 - \frac{1}{ah\sqrt{\pi t}}\right]$$

$$\text{or, } R = q - [mk + Sk/h\sqrt{\pi t}] \qquad (11)$$

In Part I (p. 64) it was assumed that $R = q = A/a$, which would be strictly true only after an infinite time. Here it is assumed that $1/n$ of the earth's heat is due to radio-activity, i.e. $R = q/n$. Hence from equation 11 we have

$$q/n = q - [mk + Sk/h\sqrt{\pi t}]$$

$$\text{or, since } q = k\, d\theta/dx$$

$$(d\theta/dx)/n = d\theta/dx - [m + S/h\sqrt{\pi t}].$$

Here the only unknown is n, which is given by

$$n = \frac{d\theta/dx}{d\theta/dx - m - S/h\sqrt{\pi t}} \qquad (12)$$

$$= 4/3.$$

This result could also be obtained as follows: without radio-activity the temperature gradient at the surface after 1,600 million years of cooling would be

$$d\theta/dx = m + S/h\sqrt{\pi t}$$

$$= 0{\cdot}00005 + 0{\cdot}00003$$

$$= 0{\cdot}00008^\circ \text{ C. per cm.}$$

But the actual gradient is $0{\cdot}00032^\circ$ C., so that three-fourths of the total present heat flow is due to radio-activity.

The above discussion clearly proves that the earth could have cooled from a state in which it was molten at the surface to its present condition (as implied by the temperature gradient at the surface) even if three-fourths of the heat flow be due to radio-activity. This interesting conclusion must, however, be tested further by reference to volcanic temperatures. If the assumptions made bear a close relation to the actual conditions, they must lead to temperatures in depth in keeping with the facts of vulcanism.

11. Bearing on the Age of the Earth.

It is interesting to notice that in the absence of radio-thermal energy the age of the earth given by the data used in this paper would be twenty-two million years, the equation

$$t = 1\Big/\frac{h^2\pi}{S^2}\left(\frac{d\theta}{dx} - m\right)^2$$

being derived from the non-radio-active part of equation 10.

[1] See equation 61, Ingersoll & Zobel, loc. cit., p. 89.

This is practically the same as the result ultimately arrived at by Kelvin (twenty to forty million years, and probably nearer twenty than forty). Ingersoll & Zobel discuss a case in which one-fourth of the earth's heat is due to radio-activity, and show that the age is then to be doubled. If it be assumed that on the average three-fourths of the heat is due to radio-activity, then the age is raised to 1,600 million years, as shown by equation 12. It should be noticed, however, that the mathematical treatment does not take into consideration the slow decrease of the radio-active generation of heat with time. For example, 400 million years after the consolidation of the crust, the gradient due to radio-activity would be about 15 per cent higher than now, and at the time of consolidation the gradient would be about 20 per cent greater than now. It thus happens that, actually, the effect of radio-activity has been to retard normal cooling in the early periods of the earth's history more effectively than the mathematical treatment would suggest. Taking this into account, it can be shown that on the average the rate of cooling has been retarded about 10 per cent more than if the heat generation due to radio-activity were independent of time. Thus, the age of the earth as measured from the consolidation of the crust, if that event ever took place, would be 1,750 million years instead of 1,600 million years, or alternately, if 1,600 million years be adopted as the maximum, the heat flow from radio-activity must be slightly less than three-fourths of the whole, say about 27/40.

If the proportion of heat flow due to radio-activity is assumed to be higher than this, the age of the earth increases with great rapidity, and we are driven to choose between two alternatives—either

(*a*) that the age of the earth is much greater than 1,600 million years, or

(*b*) that the earth has never possessed a molten or even a nearly molten crust.

(*a*) cannot be readily granted, and therefore the practical alternatives lie between

(*b*) with more radio-thermal energy than is necessary to supply three-fourths of the total heat loss, and

(*c*) the conception of a cooling earth which originally had a molten crust and in which radio-activity supplies three-fourths, or nearly three-fourths of the present heat loss.

12. Deep-seated Temperatures.

The temperature θ at any depth x, and at any time t, is given by Ingersoll & Zobel's equation 85. Theoretically, the "steady state" in which all the heat from radio-active sources passes to the surface is arrived at only after an infinite time, and equation 85 then reduces to

$$\theta = mx + A/a^2k\,[1 - e^{-ax}].$$

Practically, however, the time t, 1,600 million years, is sufficiently long for the steady state to be assumed without appreciable error, and the temperature at any depth may thus be regarded as made up of two components, one, θ'', due to the initial thermal condition of the

earth, and the other, θ', due to radio-active disintegration. From Ingersoll & Zobel's equation 60 (p. 89) we find—

$$\theta'' = mx + S \cdot \frac{2}{\sqrt{\pi}} \int_0^{\frac{x}{2h\sqrt{t}}} e^{-\beta^2} d\beta \qquad (13)$$

The evaluation of the "probability integral" attached to S can be obtained for different values of $x/2h\sqrt{t}$ $(=0{\cdot}00263x)$ from a table on p. 166 of Ingersoll & Zobel's book. The equation for the other component θ' has already been given in Part I, p. 69, as equation (9):

$$\theta' = A/a^2k\left[1-e^{-ax}\right] \qquad (9)$$

From 13 and 9 the total temperature $\theta=\theta'+\theta''$, at any depth x, can be calculated. The results for various depths are computed in Table V. It should be noticed that A/a^2k, the maximum possible temperature due to radio-activity, is 800° C.

TABLE IV.

Depth.	Equation 13.				Equation 9.			Total Temperature.
x in kms.	mx° C.	Probability, integral, p	Sp. (°C.)	θ'' (°C.)	e^{-ax}	$1-e^{-ax}$	θ' (°C.)	$\theta=\theta'+\theta''$
10	50	0·029	29	79	0·6704	0·6296	264	343° C.
20	100	0·059	59	159	0·4493	0·5507	441	600° C.
30	150	0·090	90	240	0·3011	0·6989	560	800° C.
40	200	0·118	118	318	0·2018	0·7982	640	958° C.
50	250	0·146	146	396	0·1353	0·8647	692	1088° C.
60	300	0·179	179	479	0·0907	0·9093	727	1206° C.
70	350	0·205	205	555	0·0608	0·9392	752	1307° C.
80	400	0·234	234	634	0·0408	0·9592	768	1402° C.
90	450	0·263	263	713	0·0273	0·9727	778	1491° C.
100	500	0·290	290	790	0·0183	0·9817	785	1575° C.

These results are highly satisfactory. In Part I, p. 70, assuming all the earth's heat to be due to radio-activity, it was shown that at a depth of 50 km. (31 miles) the most probable average temperature would be about 1100° C. Now, assuming that, in spite of radio-activity, the earth's crust has cooled down from an initial molten state, we find the temperature at a depth of 50 km. to be 1088° C. Moreover, in this case, the temperature continues to rise as the depth increases, so that, for example, at 100 kilometres the temperature is 1575° C. If all the earth's heat is due to radio-activity the maximum possible temperature is not likely to be more than 1200° C. It therefore appears that the assumption of a cooling earth leads to results more in keeping with the thermal necessities of igneous activity than does the assumption that the earth is in thermal equilibrium, its heat being maintained wholly by radio-thermal energy. It cannot be argued from this that the earth's crust *has* been molten, but it may be safely concluded that if other geological evidence points to, or requires, an initial molten crust, then there is nothing in the distribution of the radio-active elements to forbid belief in such a hypothesis.

13. Conclusions of Part II.

1. The effect of the generation of radio-thermal energy in the earth's crust is to slow down the normal rate of cooling, and it is shown that if the earth had originally a temperature of 1000° C., at, or just below the surface, the effect of radio-activity sufficient to maintain nearly three-fourths of the present flow of heat would be to increase the period of cooling from twenty-two million years to 1,600 million years.

2. The method of estimating geological time by lead-uranium ratios in radio-active minerals indicates that the oldest igneous rocks of the earth's crust have an age of about 1,500 million years.

3. Geological and other evidence points favourably to the traditional view that the earth's crust was initially in a molten state.

4. If the earth has cooled down from such a state to its present thermal condition, nearly three-fourths of the present heat output must be supplied by radio-activity.

5. Volcanic temperatures can be more satisfactorily obtained in depth on this view (4) than if the whole of the present temperature gradient is maintained by radio-activity (Part I).

6. There is nothing in the distribution of the radio-active elements either superficially or in depth to forbid belief in an earth which began with a molten surface, and which has gradually cooled down to its present condition.

Finally, I wish to express my thanks to Dr. Evans for valuable suggestions and salutary criticisms offered during many discussions on the subject-matter of this paper.

III.—Some Palæozoic Fossils referred to the Cirripedia.

By Thomas H. Withers, F.G.S.

SCATTERED throughout the Palæozoic rocks there are frequently found certain fossils, in most cases represented by single detached plates, which have been ascribed by various authors to the sub-class Cirripedia. Thus we have fossils described under the names *Balanus*, *Cirripodites*, *Lepidocoleus*, *Plumulites*, *Pollicipes*, *Protobalanus*, *Scalpellum*, *Stenotheca*, *Strobilepas*, and *Turrilepas*, but in the case of *Balanus*, *Pollicipes*, and *Scalpellum*, the reference to those recent genera is quite unsupported by the structure of the fossils.

While the Cirripede nature of some of these fossils appears to be undoubted, others as certainly are not Cirripedes, and for the remainder it would seem that, except for the similarity in ornament to the valves of Cirripedes, the main reason for such reference is the great difficulty in referring them to any other class of animals.

It is only occasionally that the plates of these forms are found in position so that we can gain some idea of what the complete shell was like, and although more or less complete specimens have been found of most of them, in no case is it possible to learn the precise relationship of the animal to the shell.

These brief notes are published in order to clear the ground for a more detailed study of some of the above-mentioned forms. I wish

especially to point out that the occurrence in Palæozoic times of the essentially recent genera *Pollicipes* and *Scalpellum* is still unproved. In a paper now in course of preparation I propose to deal in more detail with the structure of *Turrilepas*, *Plumulites*, and *Lepidocoleus*.

Balanus.

Under the name *Balanus carbonarius*, Petzholdt (*De Balano et Calamosyringe, Additamenta ad Saxoniæ Palæologiam duo*, 8vo, *Dresdæ* and *Lipsiæ*, 1841, p. 5, pl. i; "Über Balanus carbonaria," Neues Jahrb., p. 403, pl. iv, 1842) has described from the Carboniferous rocks near Dresden, Saxony, a group of fossils which, from the description and figure, it is impossible to accept as belonging to the genus *Balanus* or even to the Cirripedia at all. Darwin (Pal. Soc. Monogr. Foss. Lepadidæ, 1851, p. 5)[1] has already pointed out that, "as neither the operculum, the structure of the shell, the number of the valves, nor their manner of growth, can be made out or are described, the evidence appears quite insufficient to admit the existence of this genus at so immensely a remote epoch." Until further evidence is forthcoming we are unable to admit the Cirripede nature of this fossil.

Protobalanus, R. P. Whitfield, and Palæocreusia, J. M. Clarke.

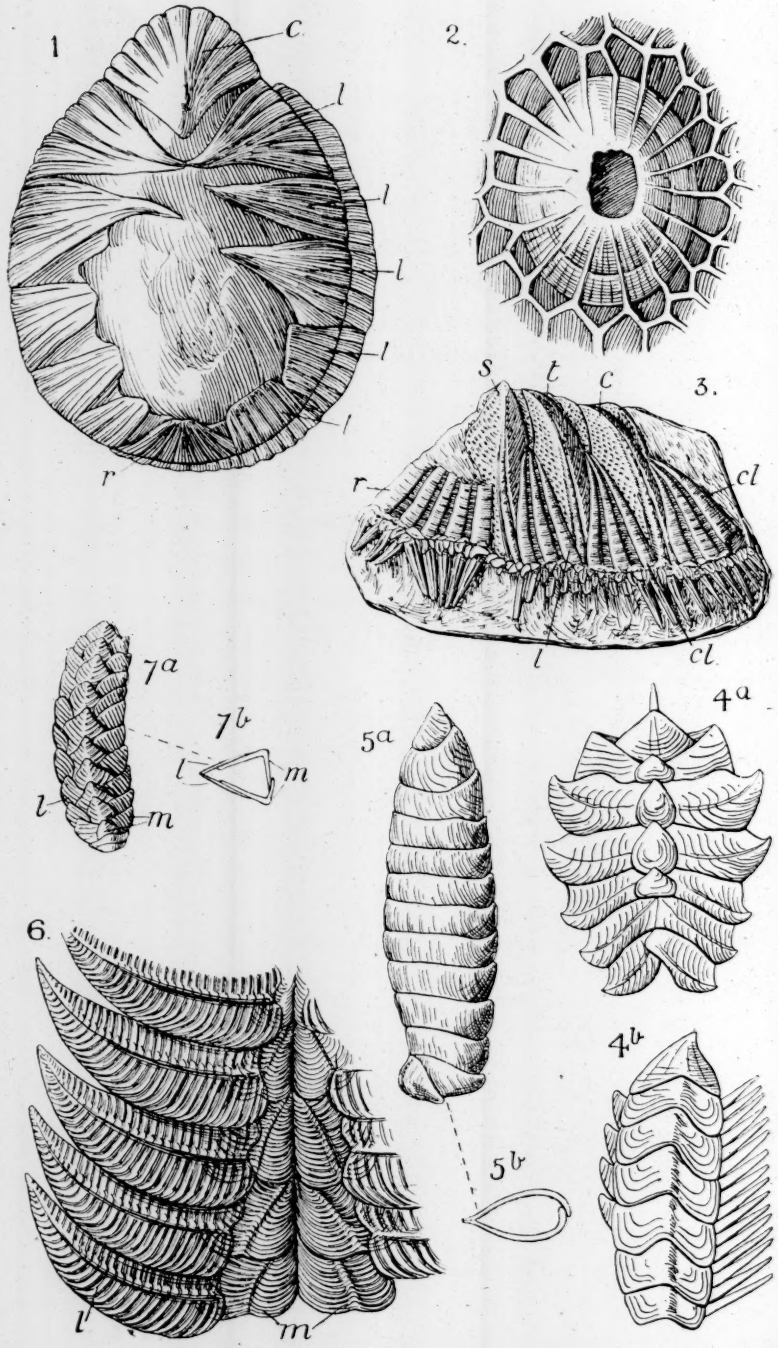
Both these forms appear to be Cirripedes and were founded on single specimens referred to the Balanidæ under the name *Protobalanus hamiltonensis*,[2] R. P. Whitfield, from the Middle Devonian, Hamilton Group (Marcellus shales) of Avon, Genessee Co., N.Y., and *Palæocreusia devonica*,[3] J. M. Clarke, from the Middle Devonian, Corniferous Limestone of Le Roy, Genessee Co., N.Y.

Protobalanus hamiltonensis (Fig. 1, p. 114) is represented by a small depressed-convex shell, ovate in general outline, narrowing towards the carinal end, and composed of twelve peripheral plates. There is a semicircular carina elevated above the other plates, a short rostrum, and five pairs of subtriangular lateralia, which are regularly disposed and symmetrical, the radial areas of the plates being conspicuous and apparently smooth. The length of the specimen is 4·5 mm., and its greatest breadth 3·5 mm. Although this form has a greater number (twelve) of plates than is the case in any of the forms of the family Balanidæ (the number in that family being eight, six, four, or, where all the plates are coalesced, one) it has certain characters in common with the members of that family. In the first place it has at each extremity plates comparable to the rostrum and carina, the carina as usual being the highest plate, and on each side the paired lateralia, which number five in *Protobalanus*, while the highest number in a member of the Balanidæ, namely *Octomeris*, is three. There are also radial areas to the plates, but it is not clear whether all the

[1] See also Darwin, Ray Soc. Monogr. sub-class Cirripedia, Balanidæ, 1854, p. 492.

[2] R. P. Whitfield, in Hall & Clarke, *Palæont. New York*, vol. vii, p. 209, pl. xxxvi, fig. 23, 1888; R. P. Whitfield, Bull. Amer. Mus. Nat. Hist., vol. ii, p. 67, pl. xiii, fig. 22, 1889.

[3] J. M. Clarke, in Hall & Clarke, *Palæont. New York*, vol. vii, p. 210, pl. xxxvi, figs. 24–6, 1888.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EXPLANATION OF ILLUSTRATION ON OPPOSITE PAGE.

FIG. 1.—*Protobalanus hamiltonensis*, R. P. Whitfield. Middle Devonian, Hamilton Group (Marcellus Shales): Avon, Genessee Co., N.Y. (Copied from figure in Hall & Clarke, *Palæont. New York*, 1888, pl. xxxvi, fig. 23.) Shell viewed from above. *c*, carina; *r*, rostrum; *l*, lateralia.

FIG. 2.—*Palæocreusia devonica*, J. M. Clarke. Middle Devonian, Corniferous Limestone: Avon, Genessee Co., N.Y. (Copied from figure in Hall & Clarke, tom. cit., pl. xxxvi, fig. 26.) Shell viewed from above, embedded in *Favosites hemisphæricus*.

FIG. 3.—*Hercolepas signatus*, Aurivillius sp. Upper Silurian, bed *e* (= Lower Ludlow): I. of Gotland. (After Aurivillius.) Side view. *c*, carina; *s*, scutum; *t*, tergum; *c.l.* carino-latera; *l*, lateral; *r*, rostrum.

FIG. 4.—*Strobilepas spinigera*, J. M. Clarke. Middle Devonian, Hamilton Group, Hamilton Shales: Canandaigua Lake, N.Y. (After J. M. Clarke.) *a*, 'dorsal view'; *b*, side view.

FIG. 5.—*Lepidocoleus sarlei*, J. M. Clarke. Middle Silurian, Niagara Shales: Rochester, N.Y. (After J. M. Clarke.) *a*, side view; *b*, diagrammatic transverse section.

FIG. 6.—*Plumulites peachi*, Nicholson & Etheridge, jun. Upper Ordovician, Drummuck Group: Whitehouse Bay, Girvan, Ayrshire. Type-specimen, figured Etheridge, jun. & Nicholson, Mon. Silur. Foss. Girvan, pl. xx, fig. 8. (After F. R. Cowper Reed.) Mr. Cowper Reed figures this as the inner view, but it seems to me to be the outer view. An incomplete individual showing the two median rows of abutting plates (*m*) and the lateral kite-shaped plates (*l*).

FIG. 7.—*Turrilepas wrightiana*, de Koninck sp. Middle Silurian, Wenlock Beds: Dudley, Worcestershire. (After H. Woodward.) *a*, basal half of an individual, showing the two median rows of intersecting keeled plates (*m*) and a row of lateral plates (*l*); *b*, diagrammatic transverse section.

plates have them, or only some; these areas appear to be smooth. No information is available as to the structure of the walls of the shell, which is so characteristic in the members of the Balanidæ. From the figure, however, it would seem that the apices of the three pairs of lateralia nearest the carina meet on a line extending from the apex of the carina to that of the rostrum. There do not appear to be any fractures due to flattening during fossilization, although a portion of the shell near the margin of the whole of the right side has been creased by compression. Since, however, the original describer states that the shell is depressed-convex, one gains the impression that the whole of the shell has not been flattened. If, therefore, the two pairs of lateralia adjoining the rostrum extended near to the median line as do the remaining unbroken lateralia, then it is difficult to understand where the opercular valves could have been situated. A re-examination of the specimen would be interesting, but it may now be said that it is exceedingly improbable that it is even remotely related to the genus *Balanus*, or to any other member of the Balanidæ.

Palæocreusia devonica (Fig. 2) is known by a single shell in which the separate plates cannot be made out; it is ovate in outline, patelliform, with the surface gently conical, and slightly depressed on the posterior slope. The wall of the shell is apparently thin, and the surface marked with faint radiating striæ or elevated lines. A conspicuous furrow extends near to and concentric with the margin. Length 10 mm., width 8·5 mm. As the author states, it brings to

mind the representatives of the recent genera *Pyrgoma* and *Creusia*, and since the generic differences from *Creusia* are not readily apparent, the term *Palæocreusia* is used tentatively to express the probability that such differences will eventually be found. It may here be pointed out that, while the recent and fossil forms included in *Pyrgoma* have the shell in one piece, the sutures of the plates rarely being seen, and then only those of the carina, in *Creusia* the four compartments with their radii are quite distinctly seen.

According to Clarke, "The specimen is attached to a colony of *Favosites hemisphæricus* and has at some time been overgrown as far as the aperture by the multiplication of the cell tubes. A portion of the coral was subsequently removed by natural causes, exposing the capitulum, but leaving the tubular basis completely enveloped. The surface of the former still bears traces of the cell walls of the coral. By removal of a portion of the coral near the side of the specimen it is found that the internal cavity is partially filled with soft decomposed chert, the remainder of the cavity filling, the capitulum and the entire coral being silicified. The internal plates, *scutum* and *tergum*, are not preserved . . . The length of the tubular basis can be measured through the aperture for 8 mm., but is probably somewhat greater."

The conspicuous furrow seen near the margin of the shell is explained as probably indicating the line of contact of the wall of the shell with that of the basis, but the furrow seems to me to be too far removed from the margin for this to be the case. In recent forms the wall of the basis is in direct contact with that of the shell at its extreme marginal edge. In Clarke's figure the walls of the coral are shown to extend on to the shell and converge towards the aperture. While this suggests that the figure is somewhat restored, it suggests also that they are ribs of the shell, and not the walls of the coral at all. Had these been the walls of *Favosites* one would have expected to see them closed, and not left open, and to have seen traces of the corallites between them. Moreover, seeing that the shell is so well preserved, one would expect that the basis also would be preserved. Despite the description of Clarke as to the supposed cavity for the tubular basis, one wonders if this could not possibly be a mollusc of the family Fissurellidæ. That family extends back in time probably to the Carboniferous, while the family, which *Palæocreusia* is considered to be a representative of, is not known earlier than the Crag (Pliocene). Mr. Tom Iredale informs me that he has often seen in the Indo-Pacific Oceans, members of the Fissurellidæ attached to, and even overgrown by coral.

Since *Pollicipes signatus*, Aurivillius (Fig. 3), from bed *e* (= Lower Ludlow) of the island of Gotland, in the opinion of the original describer, shows a closer approach to the Balanidæ than do any other of the Lepadidæ, it may more naturally be dealt with here. Only a single specimen is known, and this certainly has the structure of a sessile Cirripede, and bears little resemblance to the capitulum of *Pollicipes*. Its structure is quite unlike that of any other known form, and it is desirable to place it in a new genus, of which the following may suffice for a provisional diagnosis:—

HERCOLEPAS,[1] gen. nov. (Fig. 3, p. 114.)

Sessile barnacles, in which the shell is composed of ten subtriangular plates, the five inner plates sculptured with fine punctæ, the five outer plates about one-third smaller, longitudinally grooved, overlapping on each side the plates of the inner series, with three rows of alternating plates encircling the base and consisting of an inner row of smooth scales, followed by a row of rudimentary spines, and an outer row of comparatively long grooved spines.

Genotype.—*Pollicipes signatus*, Aurivillius.

Aurivillius has given a very careful and detailed description of *Pollicipes signatus*, and in the absence of an examination of the specimen I do not propose to attempt any redescription of it here. In the figure seven large plates are seen, the three inner plates, counting from the left, being called the scutum, tergum, and carina, while the four outer plates are called the rostrum, lateral, and two carino-lateral plates. Those of the inner series are apparently all of the same shape; their outer surface is covered with minute punctæ, and they have an apico-basal ridge. Those of the outer series are altogether different in appearance, for they are about one-third smaller, and with the exception of the rostrum which has more, the plates have three longitudinal grooves with transverse ridges, the rest of the plate standing out as broad longitudinal folds. As Aurivillius has pointed out, the ornament resembles somewhat that of the Cretaceous *Pollicipes cancellatus*, Marsson (=*Brachylepas naissanti*, Hébert sp., see GEOL. MAG., July and August, 1912). In *Brachylepas*, however, the rostrum and carina have a similar external ornament, as is the case in all Cirripedes with which I am acquainted among either sessile or pedunculate forms; but, if we are to believe Aurivillius' interpretation of the valves in *P. signatus*, the rostrum and carina not only differ greatly in ornament and structure, but belong to different series. It may be impossible to see in the specimen any characters that would guide us in determining which valves are homologous with the scuta, terga, and carina, etc., in other Cirripedes, but it might have helped to give a better idea as to the identity of the valves and their disposition if a view of the specimen from above had been given. At present I am not satisfied with the identification of the valves as given by Aurivillius.

Hercolepas, *Protobalanus*, and *Palæocreusia*, appear to be the only Palæozoic forms that can be referred with any justification to the Cirripedia. *Hercolepas* is an undoubted Cirripede, and while there is very little doubt with regard to *Protobalanus*, there is more doubt in the case of *Palæocreusia*. However this may be, it is of interest to note that all these are sessile forms. With the exception of the supposed attachment scars of Balani recorded by Charles Moore (Quart. Journ. Geol. Soc., vol. xxvi, p. 243, 1870), on shells from Wollumbilla, Queensland, and the problematic '*Balanus carbonarius*' of Petzholdt (see p. 113) from the Carboniferous rocks near Dresden, Saxony, no other record of a sessile Cirripede is known until the Upper Cretaceous.

[1] ἕρκος = an enclosing fence.

The recent sessile Cirripedes are included in the families Balanidæ and Verrucidæ, the latter family consisting of the asymmetrical Cirripedes forming the single genus *Verruca*. *Verruca* extends back in time to the Cretaceous (Upper Senonian), and I have recently maintained (Proc. Zool. Soc., December, 1914, pp. 950 et sqq.) that it was derived from the more primitive form *Proverruca*, occurring also in the Cretaceous (Lower Senonian). In its structure *Proverruca* shows quite conclusively that it must have been derived from a symmetrical pedunculate Cirripede such as *Calantica* (*Scillælepas*), so that the Verrucidæ at any rate were derived from a stalked form.

The Balanidæ consists of the sub-families Balaninæ and Chthamalinæ. We have some evidence to support the supposition that the Chthamalinæ (*Catophragmus*) have arisen from some such form as the Cretaceous (Upper Senonian) sessile Cirripede *Brachylepas*, which in all probability was an offshoot from the stalked Cirripedes forming the genus *Pycnolepas*[1] (Cretaceous, Albian, to Miocene, Helvetian). On the other hand we cannot derive the Balaninæ from that source or from any known form in either the Cretaceous or Jurassic rocks.

Since, however, both the Verrucidæ and the Chthamalinæ can be traced back to the stalked forms existing in the Cretaceous, it seems very unlikely that the remaining 'sessile' group among the recent Cirripedes, the Balaninæ, can have any close relationship with the 'sessile' Palæozoic forms. It is much more probable that the ancestors of the Balaninæ are to be sought for in the Upper Cretaceous or early Tertiary rocks. Further, while among recent Cirripedes the sessile condition is undoubtedly due to a secondary modification of the pedunculate type, it must be kept in mind as a possibility that this may not have been the case among the more primitive forms of the Palæozoic rocks. We can as yet form only a vague idea of the ancestral Cirripede, but there is no need to assume that it possessed a peduncle.

Cirripodites, G. F. Matthew, and Stenotheca, J. W. Salter.

In a paper on the "Faunas of the Paradoxides Beds in Eastern North America, No. 1" (Trans. N.Y. Acad. Sci., vol. xv, pp. 200–7, 1896), Dr. G. F. Matthew has described from the Cambrian certain scattered plates which he ascribes to a new genus *Cirripodites*, and to the genus *Stenotheca*, Salter.[2] The latter genus was found by Salter to include a minute, cap-shaped shell, which he considered to be a Pteropod, but which is now classed among the Gastropods. Even if the forms described by Matthew are not Pteropods or Gastropods, I can see no valid reason for their reference to the Cirripedia, and the comparative thickness of certain of the plates is certainly not in favour of this reference. With regard to the plates included in *Cirripodites*, I fail to see any characters in common with any Cirripede, and I find no reason for dissenting from Matthew's own opinion that "The reference of these plates to Cirripedes is largely a matter of conjecture".

[1] See T. H. Withers, "Some Cretaceous and Tertiary Cirripedes referred to *Pollicipes*": Ann. Mag. Nat. Hist., August, 1914, pp. 199 et sqq.

[2] J. W. Salter in H. Hicks, Quart. Journ. Geol. Soc., vol. xxviii, p. 180, 1872; J. W. Salter, Cat. Camb. Sil. Foss. Mus. Camb., p. 8, 1873.

Scalpellum, Leach, and Pollicipes, Leach.

So far as I am aware only two authors have referred any fossils from the Palæozoic rocks to either of these two genera. In a paper, "Über einige Ober-silurische Cirripeden aus Gotland," Bihang Svenska Vet.-Akad. Handl., Bd. xviii, Afd. iv, No. 3, 1892, Professor C. W. S. Aurivillius describes seven species of *Scalpellum*, namely, *S. cylindricum*, *S. fragile*, *S. granulatum*, *S. procerum*, *S. strobiloides*, *S. sulcatum*, *S. varium*, all from bed *e* (= Wenlock shale) of the island of Gotland. These seven species were all founded on specimens considered to be peduncles, but even apart from the fact that not a single plate of the capitulum had been found with them or even recorded from the same beds, there are many characters that would make one hesitate to refer them to the Cirripedia, let alone to the genus *Scalpellum*. Consequently I have never looked upon them as Cirripedes, and on showing to Dr. F. A. Bather similar fossils from the Silurian of Shropshire as very like some fossils he was working at, he recognized them as the turrets of certain forms of Edrioasteroidea. It was because of their identity in structure with the fossils described by Aurivillius that the Shropshire specimens were at first placed among the Cirripedes in the Geological Department of the British Museum. Dr. Bather has dealt with these forms in his "Studies on Edrioasteroidea" (Geological Magazine, February, 1915, p. 49).

Aurivillius further described and figured (1892, p. 12, fig. 9 of pl.), under the name *Pollicipes validus*, a fossil which he considered to be part of a scutum. It is, indeed, difficult to recognize this as a scutum of *Pollicipes*, or even of a Cirripede. The only suggestion I venture to offer is that it may be a portion of the shell, viewed from the so-called ventral margin, of a species of *Lepidocoleus*, a genus which may or may not belong to the Cirripedia. Although I was fully aware that the specific name was preoccupied by *Pollicipes validus*,[1] Steenstrup, from the Chalk (Danian) of Denmark, I refrained from giving it a name until it was possible to determine from the specimen itself what it really was. Professor J. C. Moberg, however, in a recent paper (Kongl. Fysiogr. Sällsk. Handl., N.F., Bd. xxvi, No. 1, p. 3, 1914), has renamed it as *P. aurivillii*, but does not offer any suggestion as to the nature of the fossil.

The remaining form described by Aurivillius is *P. signatus* from bed *e* (= Lower Ludlow) of the island of Gotland, a form already dealt with on p. 114, Fig. 3.

Dr. R. Ruedemann is the second author to describe Palæozoic fossils as belonging to the genus *Pollicipes*, and in a valuable memoir on the "Hudson River Beds near Albany and their taxonomic equivalents", Bull. N.Y. State Museum, No. 42, April, 1901, he describes and figures (p. 578, pl. ii, figs. 16–24)[2] a number of peculiarly shaped plates found in the Upper and Lower Utica Shale of Green Island, Mechanicsville, N.Y. He considers that these find their homologues in parts of the capitula of *Scalpellum* and *Pollicipes*, notably the latter. For this reason he unites them under the name

[1] Now referred to *Calantica* (*Scillælepas*), Withers, Ann. Mag. Nat. Hist., August, 1914, p. 196.

[2] See also R. Ruedemann, Bull. N.Y. State Mus., No. 162, p. 122, 1912.

Pollicipes siluricus. Since I have not had the opportunity of examining these specimens, and in view of the various shapes of the plates, I do not wish to throw any doubt on their Cirripede nature. It is quite a different matter with regard to their generic determination, and in my opinion the characters shown are quite insufficient for referring them either to *Pollicipes* or *Scalpellum*.

In a recent paper ("Some Cretaceous and Tertiary Cirripedes referred to *Pollicipes*": Ann. Mag. Nat. Hist., August, 1914) I pointed out that, except in the case of the more modified forms of *Scalpellum*, it was difficult to determine from separate valves the proper systematic position of the fossil. This is evidenced by the fact that Darwin and later authors included in *Pollicipes* all the forms that could not be referred to *Scalpellum*, but now that we have better material some have been included in new genera, and others have been shown to be primitive forms of *Scalpellum* (*sensu lato*). Judging from the structure of the remaining Cretaceous forms, there are very few that could possibly belong to *Pollicipes*, and in no case can it be stated positively. This is equally true of the Jurassic species, except that the possibility of their belonging to *Pollicipes* is more remote, and is reduced to fewer species. It is much more likely that many of the Jurassic and Cretaceous species are primitive forms of *Scalpellum*, but some probably belong to other genera. Notwithstanding this, it is still advisable to retain the generic name *Pollicipes* for those Jurassic and Cretaceous species which have been referred to that genus, and are represented by valves insufficient for their more correct determination.

While it may be true that certain forms existed in the Jurassic and Cretaceous rocks, or even in the Palæozoic, in which the valves had apical umbones, and therefore like *Pollicipes* had their growth directed downwards, it does not follow that they belong to *Pollicipes*. They may even, like *Pollicipes*, have had a larger number of valves than *Scalpellum*, namely eighteen to over a hundred, but the fact is that, now we know more of what the complete shell in certain fossil forms was like, we see that the difference lies essentially in the disposition of the valves, not in their number.

On phylogenetic grounds I consider it to be highly improbable, if not impossible, that the genus *Pollicipes*, which is probably a polyphyletic one, and still more *Scalpellum*, could have existed in Palæozoic times, and it is not sufficient to produce a valve with an apical umbo to prove the existence of *Pollicipes*. It is necessary to piece together the greater part of the capitulum before we can gain any idea of the affinities of any form. Apart from this, however, the valves hitherto referred to *Pollicipes* and *Scalpellum* from Palæozoic rocks do not even conform to the old conception of the valves of those genera, for none of the valves has a structure closely approaching even the Jurassic and Cretaceous forms.

Turrilepas, H. Woodward, Plumulites, Barrande, Lepidocoleus, C. L. Faber, and Strobilepas, J. M. Clarke.

Since Dr. H. Woodward[1] referred the fossil from the Wenlock Shale of Dudley, originally described by de Koninck as *Chiton*

[1] H. Woodward, Quart. Journ. Geol. Soc. London, vol. xxi, p. 486, 1865.

wrightianus,[1] to the Cirripedia under the new genus *Turrilepas*, many fossils from the Palæozoic rocks have been referred to the Cirripedia under the names *Turrilepas*, *Plumulites*, *Lepidocoleus*, and *Strobilepas*. While it is doubtful if certain of the fossils even belong to these genera, it certainly still remains to be proved that any of them belong to the Cirripedia.

Strobilepas is known only by one species, *S. spinigera*, J. M. Clarke (Fig. 4, p. 114),[2] based on a single specimen from the Middle Devonian (Hamilton Group) of New York, and in this specimen the plates are somewhat displaced. J. M. Clarke (1888, p. lxiii) has given a restoration of it, and from this we learn that there are four columns of plates, of which the outer two are large and of equal size. One of the two intervening columns consists of a few very small plates, and the other is modified into a series of spines, which lie opposite the series of small plates. The apical extremity is terminated by a circular, conical plate, against the sides of which lies the first plate in each column. It appears to form a completely enclosed shell, and is less like a Cirripede than the other genera, for if it be a Cirripede it is difficult to imagine where the cirri could have protruded in search of food.

Lepidocoleus is known by several species, namely the genotype *L. jamesi*,[3] Hall & Whitfield (originally described as *Plumulites jamesi*[4]), from the Hudson River Group of Cincinnati, *L. sarlei*,[5] J. M. Clarke (Fig. 5, p. 114), from Niagara Shales of New York, *L. polypetalus*,[6] J. M. Clarke, from the Lower Helderberg Group of New York, *L. illinoiensis*,[7] Savage, from the Upper Oriskany Group of Illinois, and a species, *L. suecicus*,[8] recently described by Professor J. C. Moberg from the Upper Ordovician of Sweden. The genus is represented also to my knowledge in the Wenlock Beds of Dudley, in the Ordovician of Bohemia, and in the Middle Devonian of Moravia. The record from Bohemia is based on four plates in the Geological Department of the British Museum, accompanied by one of Barrande's original labels, which reads "*Squamula bohemica*, Barr. D-. Mt. Kosow". Barrande states in his Monograph (*Syst. Silur. Bohême*, vol. i, Suppl., p. 565, 1872) that in 1856 certain fossils were distributed to the British Museum under the generic names *Plumulites*, *Anatifopsis*, and *Squamula*. He later, however, considered that there were two generic types only, not feeling justified in retaining the proposed genus

[1] De Koninck, Bull. Acad. Sci. Belgique, ser. II, tom. iii, p. 190, 1857.

[2] J. M. Clarke in Hall & Clarke, *Palæont. New York*, vol. vii, p. 212, pl. xxxvi, figs. 20–2, 1888.

[3] C. L. Faber, Journ. Cincinnati Soc. Nat. Hist., vol. ix, p. 15, pl. i, figs. A–F, 1886.

[4] J. Hall & R. P. Whitfield, Geol. Surv. Ohio, *Palæontology*, vol. ii, p. 106, pl. iv, figs. 1, 2 (*non* fig. 3 = *Turrilepas wrightiana*), 1875.

[5] J. M. Clarke, *Amer. Geol.*, vol. xvii, p. 143, pl. vii, figs. 1–6, 1896.

[6] Ibid., figs. 7–8.

[7] T. E. Savage, *Amer. Journ. Sci.* (4), vol. xxxv, p. 149, figs. 1–3, 1913.

[8] J. C. Moberg, "Om Svenska Silurcirripeder": Kongl. Fysiogr. Sällsk. Handl., N.F., Bd. xxvi, No. 1, p. 13, pl. ii, figs. 1–11, 1914. Since the present paper was sent to press Professor Moberg has published some additional notes, Geol. Fören. Stockholm Förhandl., Bd. xxxvi, Hft. vi, p. 492, 1915, which in no way affect what is here written.

Squamula. The above four specimens are therefore some of those which he originally intended to include in *Squamula*, and they appear to be identical with the plates figured by him (p. 576, pl. xx, figs. 22–4, 1872, especially fig. 22) as *Plumulites squamatula*, a species recorded from Étage D and E. Their reference to *Plumulites* was probably the reason for giving up *Squamula*, but that genus would have been quite justified since *Plumulites squamatula* undoubtedly belongs to the genus *Lepidocoleus*, C. L. Faber (1887). Meanwhile the name *Squamula* has no nomenclatorial validity.

In *Lepidocoleus* there are only two vertical columns of plates apparently overlapping on the median line, with slight alternation, and the shell is capable of opening along the narrow free edge.

Turrilepas (Fig. 7, p. 114) and *Plumulites* (Fig. 6, p. 114). These two genera differ from *Lepidocoleus* in having a greater number of vertical rows of plates. Of the actual number there is some difference of opinion, but since I am able to state positively that there are only four in *Turrilepas*, it is probable that there are four rows also in *Plumulites*. Much misconception has arisen with regard to these two genera, mainly owing to the fact that the structure of them has been misunderstood, with the result that while many species have been distributed between them, some authors give priority to *Plumulites* and others to *Turrilepas*. The whole history need not here be gone into, but it is clear that these two genera are quite different. The main difference lies in the two median rows of plates, for while in *Turrilepas* (Fig. 7) they are keeled or sharply bent longitudinally, and alternate with each other, in *Plumulites* (Fig. 6) the plates are not keeled, are altogether different in shape, and do not alternate, their inner margins abutting. The plates on each side of the keeled plates in *Turrilepas* are not minute as described by Dr. Woodward and Professor Moberg, but are about the same size as the keeled plates. They resemble the leaf-like plates in *Plumulites*, except that they are not so much produced at their apical ends and do not possess a median longitudinal fold. While it is gratifying to note that Professor Moberg in his above-quoted paper, "Om Svenska Silurcirripeder," has recognized the difference between *Plumulites* and *Turrilepas*, a view I have held for some time, it may be said that it is not British palæontologists alone who have confused these two genera. Everyone who has written on these forms has done so, and the authors of the genera even claimed priority each for his own genus, thinking that they were synonymous. But Mr. F. R. Cowper Reed[1] doubted whether they were synonymous, although they are so regarded even in the last edition of Zittel–Eastman. Professor Moberg conveniently gives on the same plate figures of all the above genera, but, seeing that he distinguishes between *Plumulites* and *Turrilepas*, it is surprising to find that he figures, not *Plumulites bohemicus*, Barrande,[2] which should be regarded as the genotype of *Plumulites*, a species very like *P. peachi* (Fig. 6), but *P. folliculum*, Barrande.

[1] F. R. C. Reed, "The Structure of *Turrilepas Peachi* and its allies": Trans. Roy. Soc. Edin., vol. xlvi, pt. iii, No. 21, p. 519, 1908.

[2] J. Barrande, *Système Silurian Bohême*, Supplement to vol. i, p. 569, pl. xx, fig. 1, 1872.

a form in which Barrande could not discern the ornament so characteristic of *Plumulites*. It is even impossible to make out the contour of the plates, or their number, which J. M. Clarke thought composed only two rows, so this form is very probably distinct from the genus *Plumulites*. The state of preservation of all the examples of *P. folliculum* as contrasted with the other forms would suggest this. There is no definite evidence that the single plate referred by Barrande (1872, pl. xx, fig. 10) to this species really does belong to it.

In conclusion, it may be said that we know very little of the relationship to one another of *Lepidocoleus*, *Plumulites*, and *Turrilepas*, and very little is known even of the structure of their shells. In none do we know the relation of the animal to the shell, and except for the ornamentation and the downward growth of the plates there seems to be no other character advanced in favour of their reference to the Cirripedia.

I wish to express my thanks to Dr. F. A. Bather and Dr. W. T. Calman for their valuable assistance with this paper.

IV.—The Work of Professor Lacroix on the Laterites of French Guinea.

By L. Leigh Fermor, D.Sc., A.R.S.M., F.G.S., Geological Survey of India.

(Concluded from the February Number, p. 82.)

Alluvial Laterites or Lateritites (Latérites alluvionaires).

ALL the phenomena hitherto noticed have been worked out for the case of rocks altered *in situ*, but they also take place in the products of transport resulting from the demolition of laterites *in situ*.

In these *lateritic alluvia* (*alluvions latéritiques*) or *lateritites* the zone of departure is the base of the alluvium itself, in which the hydrolysis of any aluminous silicates still remaining is continued, whilst the upper part forms a ferruginous cuirass, differing from that of the non-transported laterites only when quartz debris or transported fragments of rock, themselves usually lateritized, are present. When the alluvium is constituted by fine particles the conditions are favourable for the formation of very regular ferruginous pisolites, and in Professor Lacroix' opinion the specimens of constant aspect so frequently seen in collections from many tropical countries probably come from such occurrences.

Lacroix adopts for these rocks my term *lateritite*, proposed for detrital or secondary laterites, which, it is seen above, are subjected to a continuance of the same processes that led to the formation of the primary laterite. (See analysis No. 16, Table II, *infra*, for a very impure lateritite with 31 per cent quartz, 30 per cent clay, and 39 per cent lateritic constituents.) He also finds in many parts of Guinea a conglomerate composed of small fragments of gibbsitic laterite, mixed with clastic products, in particular quartz, derived from neighbouring rocks, and constituting a true sedimentary rock intermediate between the type described in the preceding paragraph and the *latérite d'allurions* to be noticed in the next paragraph.

It should be noted here, although it is not so stated by Lacroix, that this conglomerate is also covered by my definition of lateritite.

Lateritized Alluvium (*Latérites d'alluvions*).

This type of laterite is perhaps the most widely developed in Guinea, existing as a continuous ferruginous crust to ordinary non-lateritic alluvium in the coastal regions of Guinea between the sea and the foot-hills of Futa-Jallon and in the plain lying between the eastern limits of these mountains and the Niger. In this type of laterite the iron is not derived from the material undergoing lateritization, but is derived from the rock underlying the alluvium, as I gathered from a personal discussion with Professor Lacroix. This iron rising in solution from the underlying rocks becomes oxidized and precipitated so as to be superposed on the clastic elements of the alluvium, which are themselves more or less unaltered. The iron is therefore of extraneous origin. For this form of laterite Lacroix proposes to use my term *lateritoid* as follows (p. 323):—

"Peut-être serait-il bon de distinguer par un nom spécial ses latérites d'alluvions de celles formées aux dépens de roches en place, et, dans ce cas, on pourrait les appeler *latéritoïdes*, en donnant de l'extension au terme proposé par M. Fermor pour désigner des roches latéritiques de l'Inde, résultant de l'envanissement de grès, de schistes, par des oxydes de fer et de manganèse et constituant souvent des minerais de ces métaux!"

My term *lateritoid* was proposed for the case of lateritic rocks formed by the metasomatic replacement of non-lateritic rocks by lateritic constituents presumed to be of extraneous origin; and in all the cases described by me the rock undergoing replacement was an old, properly consolidated rock. Professor Lacroix' 'lateritoid' was formed at the expense of a recent alluvium, by the introduction of extraneous iron oxide, and should be regarded as a lateritoid only if the iron oxide has introduced itself by metasomatic replacement into the alluvium and has not merely superposed itself on the latter by filling up interstices.

Professor Lacroix assures me that the introduction of iron has been effected by a replacement of the cement between the grains of quartz in the alluvium, the alumina in the *latérite d'alluvions* being derived from the decomposition of the aluminous silicates in the cement. In India the quartz of a quartzite may suffer complete replacement by iron or manganese oxides with formation of lateritoid, but in the Guinea *latérite d'alluvions* the quartz itself does not appear to have suffered replacement, and it is evident that the two cases are not strictly comparable as regards mode of formation. In Table II of this article are shown two analyses (Nos. 14 and 15) of *latérites d'alluvions* (lateritized alluvium) from Guinea. Professor Lacroix excludes 24·51 per cent of quartz from No. 14 and 67·39 per cent of the same constituent from No. 15, so that No. 14 appears to contain 63 per cent of lateritic constituents and 35 per cent of clay, and No. 15 to contain 79 per cent of lateritic constituents and 21 per cent of clay. This gives a misleading impression, and in calculating the mineralogical compositions (shown in the same table)

TABLE II.—ANALYSES OF LATERITES DERIVED FROM MICA-SCHIST AND ALLUVIUM.

Original Rocks.	Mica-schist.			Alluvium.		Laterite, etc.
Locality.	Siguiri.	Fatoya.		Not stated.	The Tinkisso.	Not stated.
Position in Section	Zone of Concretion	Zone of Concretion	Zone of Concretion (cuirass).	(?) Crust.	(?) Crust.	Zone of leaching.
Name of Rock.	Pisolitic bauxitic laterite.	Quartzose aluminous laterite.	Pisolitic bauxitic laterite.	Lateritized alluvium or siliceous 'lateritoid'	Lateritic alluvium.	Lateritite.
Number.	11	12	13	14	15	16
SiO_2	0·72	1·22	4·37	16·85	9·91	13·12
TiO_2	1·02	0·19	0·66	0·46	1·44	1·68
Al_2O_3	50·62	46·31	39·79	22·99	22·41	25·61
Fe_2O_3	17·69	17·65	32·26	42·85	52·53	12·97
CaO	0·17	0·19	0·17	0·07	tr.	0·13
MgO	—	—	—	—	0·43	0·04
H_2O	25·80	21·40	21·05	16·78	13·26	15·81
Quartz	6·23	13·33	1·70	(24·51)	(67·39)	30·58
	102·25	100·29	100·00	100·00	99·98	99·94
Mineral Composition :—						
$Al_2O_3 . 2SiO_2 . nH_2O$ [1]	1·55(2)	2·6 (2)	9·4 (2)	27·4 (2)	6·5 (1)	30·2 (3)
bauxite: $Al_2O_3 . 3H_2O$ (colloidal) [2]	58·6	43·4	33·7	9·4	—	22·1
bauxite: $Al_2O_3 . H_2O$ (colloidal)	13·9	19·9	16·5	0·5	5·4	—
$2Fe_2O_3 . 3H_2O$ (limonite and stilpnosiderite) [3]	20·7	20·6	37·7	37·8	20·1	15·15
$TiO_2 . H_2O$	1·25	0·2	0·8	0·4	0·6	2·1
Quartz	6·2	13·3	1·7	24·5	67·4	30·6
	102·2	100·0	99·8	100·0	100·0	100·15

Notes.—Mineral compositions recalculated by me because Lacroix takes no account of quartz and titania. There must be a misprint in the first analysis.

[1] The value of n is shown in brackets in each case, n being 2 per kaolinite. In analysis No. 16, 0·30 % H_2O has been added to bring n up to 3.

[2] In analyses 12 and 13 the hydrate is gibbsite, and not colloidal.

[3] In analyses 11, 12, and 13, some hematite is present, so that the amount of $Al_2O_3 . H_2O$ must be less than that shown.

of these rocks I have reinstated the quartz, as it is impossible otherwise clearly to understand the case. No. 15 is then seen to contain only 26 per cent of lateritic constituents, and the question of the application of the term lateritoid does not arise. This rock is clearly only a lateritic sandy alluvium. No. 14 shows 48 per cent of lateritic constituents, and is therefore on the border-line between a *lateritic alluvium* and a *siliceous lateritoid*, if the same limiting percentages be adopted for the lateritoids as for the true laterites.

Should examples be found in which the replacement has progressed further with production of rocks carrying over 50 per cent of lateritic constituents when quartz is taken into account, then the term *lateritoid* might be applied, especially if the replacement has affected the quartz itself. At present all that can be admitted on the evidence is that these *latérites d'alluvions* from Guinea tend towards conversion into a particular variety of lateritoid. They are perhaps best designated *lateritized alluvium*. The consideration of this case shows the undesirability of disregarding quartz in classifying laterites as proposed by Professor Lacroix.

V. Conditions of Formation of Laterite.

On approaching the problem of the origin of laterite, the first thought that strikes one is that there is an apparently fundamental difference between the modes of decomposition of aluminous silicates in temperate and in tropical regions. For in *temperate regions* the end product is a *clay*, in which the original aluminous silicates are represented by hydrated aluminium silicates approximating in composition to kaolinite, whilst in the *tropics* the end product is *laterite*, in which the original aluminous silicate is represented by a mixture of hydrated aluminium oxides. The two processes may be expressed by the following chemical equations:—

(1) $K_2O.Al_2O_3.6SiO_2 + CO_2 + 2H_2O = Al_2O_3.2SiO_2.2H_2O + K_2CO_3 + 4SiO_2$.
Orthoclase. Kaolinite.

(2) $Al_2O_3.2SiO_2.2H_2O + H_2O = Al_2O_3.(2 \pm 1)H_2O + 2SiO_2$.
Kaolinite. Bauxite.

Reaction No. 1 belongs to the group of changes characteristic of Van Hise's zone of katamorphism, and is doubtless of exothermic character, so that it should take place in both temperate and tropical climates, as is the case. Reaction No. 2 is a further stage in the process of decomposition, namely, a hydrolysis of the hydrated aluminium silicate with formation of the hydrated aluminium oxides characteristic of laterite. The thermal character of this second reaction is of great importance. If it be exothermic, then it is possible to regard it as representing a further stage in the changes expressed in equation No. 1, to be promoted possibly by the same agents; whilst if it be endothermic it will require for its promotion an external supply of heat. There are apparently no experimental data bearing on this question, but from inspection of the equation there seems to be no obvious reason why its thermal character should be different from that of equation No. 1, as each of them involve the liberation of silica from a silicate.

Sir Thomas Holland, however, evidently regards the second reaction as endothermic, for he speaks of a reversal of the chemical reaction.[1] The higher temperatures of the tropics as compared with the temperate regions would properly not be sufficient to produce this reversal, and therefore Holland, seeking for the operation of some other form of energy that could produce exothermic results, suggested the vital influence of life in the form of bacteria,[2]

[1] Trans. Inst. Min. Met., xix, p. 454, 1910.
[2] Geol. Mag., 1903, pp. 61–4.

by which he meant, according to a later explanation, not bacteria of any specific kind, but vegetable life of all sorts, of which the lowest forms would be the most potent, because the most abundant. Vegetable life is vastly more active in the tropics than in temperate regions, and, as pointed out by Holland, owing to the absence of a true winter in the tropics, it is not compelled to pass a part of the year in enforced inactivity, as in temperate regions. Holland's suggestion is undoubtedly a valuable contribution to the problem, and cannot be rejected as long as the thermal character of the second equation remains doubtful; or if the equation be truly endothermic, until some other agent be found more capable of providing the energy required.

Although, roughly speaking, clays and laterites may be regarded as characteristic respectively of temperate and tropical climes, yet they are not confined to their respective regions. True clays are found very commonly in the tropics; e.g. in India we have them in abundance in the Nilgiri Hills as weathering products of the charnockite series, whilst they are associated with laterite in many parts of India. Lacroix in this memoir has, as already noted, shown that the aluminous silicates of the original rocks sometimes pass directly into gibbsite, and sometimes change first into a hydrated aluminium silicate, which passes afterwards into hydrated oxides. Furthermore, bauxitic oxides are known to occur in variable amounts in the clays of temperate regions, and on investigation will probably be found to be more widely and commonly distributed therein than was suspected until recently. In a paper published about the same time as Professor Lacroix' monograph, Mr. M. G. Edwards[1] gives the results of the examination of a large series of analyses of clays of the United States. Out of 244 analyses in which the free and combined silica had been separated, 108, or 44 per cent, showed an excess of alumina above that required for the kaolinite ratio of SiO_2 to Al_2O_3 of 2 : 1. The percentage of bauxite thus detected in these clays ranges from 0·54 to 43·5; and that the distribution of these bauxitic clays is not a function of latitude is shown by the fact that two analyses from Florida, the most southern state, show an average of only 0·8 per cent of bauxite, thirty-six analyses from Georgia show an average of 11·8 per cent of bauxite, and twelve analyses from Iowa, a northern state, an average of 31·7 per cent of bauxite. There are also southern states with a high percentage of bauxite in their clays, and northern states with a low percentage.[2]

It is evident, therefore, that neither form of decomposition is confined to its characteristic region, as would be expected were both equations of the same thermal character. Professor Lacroix refers to Holland's hypothesis, but without referring to the thermal aspects of the problem he reasons from the continuity he has established in certain cases between the formation of clay and of laterite, and thinks it more natural to regard both modes of alteration as due to reactions of the same order, manifesting themselves with greater intensity in tropical countries because of special climatic conditions. He does

[1] "The Occurrence of Aluminium Hydrates in Clays": *Economic Geology*, ix, pp. 112–21, 1914.

[2] See also H. Ries, op. cit., p. 402, 1914, for criticisms of Edwards' paper.

not, however, go further into this matter as he thinks the solution of the problem of the origin of laterite should come from the experimental side, and that it would be profitless to discuss this question again without the support of laboratory experiments.[1] Instead, the author prefers to state the conditions under which lateritization takes place and those under which it does not.

In the first place it is evident, Professor Lacroix thinks, that *lateritization is not the result of a direct attack by the atmosphere*, nor by running rain-water, as is shown by the freshness of basic and syenitic rocks in cliffs and denuded surfaces.[2] On the contrary, *lateritization is everywhere intense where the slope of the ground is low enough to permit the infiltration of water and allow it to remain for a long time in contact with the rocks.* One must also give great weight, even according to Professor Lacroix, to the *action of vegetation*, which develops with extreme rapidity under such conditions, so that one cannot accept the idea recently advanced by Vageler[3] that the clayey decomposition of temperate climes is due to the action of humic acids, whilst in tropical countries the silicates undergo hydrolysis, due to the rarity or absence of humus.

For the development of a ferruginous cuirass the most favourable topographical forms are certainly the tabular plateaux provided by diabase flows, and valley-bottoms of very low gradient. In proportion as superficial concretion progresses the conditions become more and more unfavourable for the existence of vegetation, which finally to a large extent disappears. If this view be correct, then *the sterility of the bowals is not the cause, but the consequence of lateritization commenced under a cover of vegetation.* It thus becomes evident that Holland and Lacroix are really in agreement in so far as they regard the action of vegetation as a factor in the formation of laterite.

The climatic conditions already referred to are those, as the author notes, to which Dr. Maclaren has attached so great an importance for explaining the laterite of India, namely *the alternation of very humid and very dry seasons.*[4] At the beginning of the wet season the water

[1] See Meigen, *Geol. Rundschau*, ii, pp. 197–207, 1911, for a recent though incomplete résumé of theories of lateritization.

[2] This first proposition is, however, not self-evident.

[3] *Fühling's Landw. Zeitung*, lix, p. 873, 1910 (Lacroix).

[4] GEOL. MAG., p. 546, 1906. In his contribution to the discussion of J. M. Campbell's paper, Trans. Inst. Min. Met., xix, p. 414, 1911, Maclaren states that while he regards an alternation of wet and dry seasons as essential in humid regions, nevertheless a truer statement may be given, as follows: "Laterization may take place in intra-tropical regions in a desiccated zone overlying a zone charged with oxidized waters, capillary attraction translating the waters from the lower to the upper zone." "The upper zone, in humid regions with heavy monsoon rainfall, where the ground water level is near the surface, may be at the surface; in more arid regions it may be as much as three feet below." This modification is to account for the laterite in arid parts of Western Australia, believed by Maclaren still to be growing. This condition, namely, alternation of wet and dry seasons, does not, however, fit all known cases. Thus, as shown by Scrivenor, there is no alternation of seasons at Malacca; but it has yet to be shown that the laterite of Malacca is still in process of formation, or at least that its formation was not initiated under different climatic conditions. For the present, however, it is necessary to admit the formation of laterite in regions of constant rainfall.

sinks into the ground, saturation being realized after some weeks; copious springs then burst forth everywhere, permitting the removal of soluble products; this must be the time when the zone of leaching grows at the base and the phenomena of *départ* or leaching are completed near the surface. At the end of the rainy season this flow gradually ceases, the ground dries, the solutions become concentrated, and are drawn up by capillarity into the upper zone; there the dissolved products are deposited little by little, the water finally evaporating at the sun-heated surface, where the precipitation and concretion of the hydrogels is completed.

The conditions of lateritization given above fit satisfactorily the mineralogical and chemical data given in Professor Lacroix' memoir; but it is not evident that the description of the happenings in the rainy season is based on personal observation, for the author's visit coincided with the winter.

VI. The Age of Laterites.

Professor Lacroix finally states his views on the age of various sorts of laterite, regarding it as probable that all the phenomena considered in his memoir are still in a state of active evolution, although no direct observation of this can be made, except in the case of ferruginous concretionary action at the surface of the cuirass. But he gives evidence to show that the process of lateritization must be a slow one. There are laterites of at least two ages, as proved by the eroded cliffs of 'fossil laterite' contrasted with the *latérite d'alluvions* of the valleys, these two types corresponding in position to our Indian high-level laterite and low-level laterite. He finally concludes that (p. 351)—

> "Le début de la latérisation en Guinée est fort ancien et date peut-être de plusieurs périodes géologiques, sans qu'il possible de songer à aucune précision en l'absence de toute formation sédimentaire datée."

In conclusion, it is perhaps permissible to regret that this admirable piece of work should contain several numerical mistakes in connexion with the analyses, and that the author has not seen fit to illustrate his memoir with at least a sketch-map of the localities mentioned, nor to separate clearly the conclusions firmly founded on observations such as the sequence of mineralogical changes characterizing lateritization from those based less securely on probability rather than on observation, such as the conclusions brought forward in Section V of this paper.

REVIEWS.

I.—A Guide to the Fossil Remains of Man in the Department of Geology and Palæontology in the British Museum (Natural History), Cromwell Road, London, S.W. With 4 plates and 12 text-figures.

DURING the last two years the lively controversies concerning the significance and interpretation of the skull-fragments found by Mr. Charles Dawson near Piltdown have bewildered and confused the

interested public no less than a not inconsiderable number of scientific men. Under these circumstances it was highly desirable that a simple statement of the facts, devoid of all technicalities, should be made available in an easily accessible form. The Guide which Dr. Smith Woodward has written supplies this want. For fourpence anyone can now obtain an admirably lucid and concise description, illustrated with a series of photographs and diagrams, of the famous Piltdown fragments, together with an account of the remains of *Pithecanthropus*, the Heidelberg jaw, and the series of representatives of the Neanderthal race. A sketch of our knowledge of the fossil Primates provides an excellent setting for the notes on the remains of early man, for it serves to explain their zoological rank and horizon.

After referring to the coming of the "Age of Mammals" the Guide describes the place of the Primates among the Mammalia, and summarizes the present state of our knowledge of the past history of the Order as it is revealed by fossilized remains of Lemuroids, Lemurs, Monkeys, and Apes. The consideration of *Pithecanthropus* serves to introduce the account of the antiquity of man. The circumstances of Mr. Dawson's discovery of *Eoanthropus* are then set forth, and an account is given of the objects found in association with it and the reasons for assigning the human remains to an early Pleistocene horizon.

The concise account of the fragments of the Piltdown skull and the excellent series of illustrations put the reader in possession of all the essential facts, and quite definitely justify the creation of a new genus of the Hominidæ to which to assign this representative of a hitherto unknown type of primitive generalized humanity.

Many books have been written within recent years with the object of explaining and interpreting the remains of fossil man, but most of them are unsatisfactory. Some of them put forward fantastic ideas as to the antiquity of man and the course of human evolution. Others, again, confuse their readers by quoting masses of conflicting statements. This small Guide, brief though it be, is perhaps the best account of the present state of our knowledge that has yet appeared. For it is a simple, sane, and easily understood statement of admitted facts and the inevitable and indisputable inferences to be drawn from them.

In a work written for the general public, complex technical points have necessarily been explained in simple language, which may perhaps at times offend the scientific purist who likes to hide his ignorance behind high-sounding technical terms—in many cases not meaning very much. For instance, exception has been taken to the statement that "certain parts [of the brain] remain scarcely more developed than they are in a modern child" (p. 14). It would perhaps have been more accurate to express this by saying that the parts of the brain of *Eoanthropus* corresponding to those areas which in the modern human being attain their full development last of all are conspicuously ill-developed. But it seems to me that in a Guide intended primarily for the general public the simpler explanation given by Dr. Smith Woodward is justifiable, and that such a criticism as I have quoted savours of pedantry. The Guide is not

a manual intended solely for experts, although experts, whether anatomists, geologists, or anthropologists, might learn a good deal from it. In fact, no more useful introduction to the study of fossil man could be put into the hands of students.

The "Conclusion" of the Guide is such an admirably concise and sane summary of the present state of our knowledge and so characteristic a sample of the quality of the whole of the work as to be worth quoting in full:—

"The general conclusion is that man, having a skeleton essentially identical with the existing one, has lived in western Europe for a long period during great changes of climate, much alteration in geographical contour, and the dying-out of numerous wild quadrupeds. He was here long before the British Isles were separated by sea from the mainland of Europe. His immediate predecessor was a form of man (the Neanderthal or Mousterian) which more nearly approached the apes in the retreating forehead, the prominence of the bony brow, and the large size of the face. The skeleton of his trunk also exhibited a combination of more ape-like features than are known in any single human skeleton of later date. Still earlier Heidelberg man, though with typically human teeth, had a much more retreating bony chin, suggestive of close relationship with the apes. Finally, Piltdown man, which is at least as old as the Heidelberg race, and probably older, had both lower jaw and front teeth as nearly as compatible with their working on a human skull of normal width. His skull, however, though with a very large face, and in some respects the most ape-like known, appears at first sight to be contrary to expectation in the steepness of its forehead and the absence of the modern ape's characteristic brow-ridges. But it must be remembered that, in accordance with a well-known law, the skull in the adult ancestral apes of Miocene times (still to be discovered) probably resembled that of the very young existing ape, not that of a full-grown individual. Just as the bony brow-ridges are acquired during the life of each individual existing ape, so the race of apes began without them, and only gradually acquired them as an adult character through successive generations. The Piltdown skull, therefore, probably resembles the skull of the truly ancestral apes much more closely than does the later Neanderthal skull, in which the bony brow-ridges may be a mark of peculiar degeneration. The *Pithecanthropus* from Java may be another degenerate."

The admirable conciseness and adequacy of this judgment appeal most strongly to those who have critically studied the conflicting mass of literature on the points at issue.

The publication of this Guide at the present time is peculiarly opportune, not only because it puts within the reach of everyone the information to enable him to discriminate between the true and the false in the recent literature relating to primitive man, much of which is distinguished by a reckless disregard of the caution and the precision of statement usually observed in scientific work, but also because it will enable everyone interested in such matters to appreciate the meaning of new discoveries which the near future will

reveal. It is a matter of common knowledge that several important discoveries of ancient human remains are even now awaiting description, and no doubt many more important fragments will come to light now that the attention of the public is being educated to the importance of saving such material as chance may reveal. Professor Graebner tells me that a few weeks before the present war was declared two complete skeletons of Neanderthal man, with animal remains and implements in association with them, were found in situ during the excavation of a railway cutting near Bonn. The remains of a Pleistocene human skeleton were recently found in South Africa; and a skeleton from East Africa, concerning which fantastic accounts appeared in the public Press a year ago, still awaits description. Even in Australia a fossilized human (child's) skull has just come to light, thirty years after it was picked up by a boundary-rider on the Talgai station (near the village of *Pilton*!), not far from Warwick on the Darling Downs of Queensland. On August 22 this specimen was exhibited and described by Professors Edgeworth David and Wilson at the recent meeting of the British Association in Australia. The skull was found in a spot rich in the remains of *Diprotodon* and other extinct marsupials. It was discovered in a position precisely similar to that in which these extinct animals occur, and its state of mineralization is identical with that of many of these fossils, now in the Brisbane Museum, from the same area. Professor David has no doubt that the human being whose skull has been thus recovered was a contemporary of the *Diprotodon* and the *Thylacoleo*. At the same meeting illness prevented Mr. Etheridge, the Director of the Australian Museum in Sydney, from demonstrating the fact that the Dingo was also a contemporary of these extinct animals. Thus we have this complementary evidence to substantiate the belief that, even before the great marsupials had become extinct, Man with his dog had ferried across "Wallace's line", even if no other straits were then open, and made his way into Australia. Quite apart from the question of its age, this fossilized Proto-Australian skull is of peculiar interest, for it is provided with exceptionally large teeth and great projecting canine teeth worn on their posterior borders (by the lower premolars), unlike any other human teeth. Professors David and Wilson intend to publish a full account of this interesting specimen in the near future.

For the proper appreciation of the precise significance of such new information as this which the future will reveal Dr. Smith Woodward's little Guide-book should be invaluable.

G. E. S.

II.—The Transportation of Débris by Running Water. By G. K. Gilbert. United States Geological Survey, Professional Paper 86. pp. 263. 1914.

THIS elaborate report embodies the results of a lengthy experimental investigation of the laws of transportation of debris by running water. The work was done in a specially equipped laboratory at the University of California, Berkeley, during the years 1907–9. Unfortunately, the artificial streams used in the

laboratory cannot be made to conform with the meanderings, variable discharge, and forms of cross-section of natural alluvial streams, and the author is obliged to admit that the primary purpose of the investigation, which was to determine for rivers the relation of the load swept along the bed to the more important controlling factors, was not accomplished. Although the gap between the river and the laboratory has not yet been bridged, much valuable information was obtained which is directly applicable to hydraulic transport and river engineering.

Streams carry debris in various ways which can be grouped together under the headings of hydraulic *suspension* and *traction*, the latter referring to what is sometimes called the bottom load. Each of these two modes of progression may be further divided according to the nature of the bottom. In natural streams the bed is usually composed of debris which is similar to the material of the load. In artificial channels, like flumes and pipes, the bed is rigid. The Berkeley experiments were designed specifically to investigate stream traction and flume traction, since there is a marked contrast between the laws controlling each case. Generally, the measurement of the suspended load in a river is only a matter of routine and patience, but the measurement of the tractional or bottom load is difficult. Knowledge of the average ratio between the suspended and the tractional load would be of great value to geologists and engineers. Humphreys and Abbot estimated the tractional load in the case of the Mississippi to be 11 per cent of the suspended load. Guérard in a study of the Rhône found the suspended load to be less than 25 per cent of the whole. Gilbert took measurements of the two loads in the Yuba River and came to the conclusion that they were approximately equal. These data are probably the best on record, but their wide variability is a serious reflection on the value of denudational studies in relation to the wearing down of the lands and to the measurement of geological time.

The factors which control the movement of the traction load are numerous and complex, for the law of control in the case of each factor is qualified by all the other factors. The slope and discharge of the stream, the form of its channel, the fineness of debris, and the velocity are all controlling factors. Of these factors most attention has been devoted to velocity, and it has been generally assumed, though erroneously, that the load carried varies with the sixth power of the velocity. This deductive law strictly refers only to the maximum size of the grains or pebbles which a given current is competent to move. The actual law is too complex for analysis and generalization. In the Berkeley experiments an attempt was made to compare capacity for traction load with the mean velocity V_m of the stream. The following empirical average results were obtained: (1) slope constant; V_m varies with discharge; capacity $\propto V_m^{3\cdot2}$. (2) discharge constant; V_m varies with slope; capacity $\propto V_m^{4}$. (3) depth constant; V_m varies with slope and discharge; capacity $\propto V_m^{3\cdot7}$. In each case the size of the debris carried provides a further interfering factor. If fine material be added to coarse the total load is increased, and even the load of coarse material is greater than before.

There is little doubt that, from the geological aspect, experiments of these kinds, while of considerable theoretical interest, serve only as a check to undue speculation. The transportation of debris by rivers, and the degrading and aggrading of their beds are phenomena which must be studied in the rivers themselves.

ARTHUR HOLMES.

III.—REPORT ON THE RECENT GREAT ERUPTION OF THE VOLCANO STROMBOLI. By FRANK A. PERRET. Smithsonian Report for 1912. Pub. 2200. pp. 285–9, pls. i–ix. 1913.

IN 1907, and again in 1912, the normal activity of Stromboli was broken by paroxysmal eruptions which indicate that this volcano is sharing in the general increase of activity of the other Mediterranean volcanoes. The 1907 eruption produced a crater 200 metres in diameter, and was marked by Strombolian explosions in which large masses of incandescent plastic lava were ejected, accompanied by clear vapours, alternating with Vulcanian explosions in which dense black volutes of ash arose from the collapsing walls as the lava column sank. The 1912 eruption increased the diameter of the crater to 300 metres. The 1907 lava, which had partly obstructed the conduit, was ejected in irregular solid blocks (olivine basalt). Enormous quantities of ash were formed directly from liquid lava, and scoriaceous lapilli of the same composition were also ejected. Strombolian and Vulcanian explosions were even more clearly defined than in 1907. The crater is now partly filled with collapsed material from the cone, which tends to confine the gases until they gather strength to break through and form an ash cloud. The author's most interesting observation is based on the hazardous experience of being completely enveloped in a cloud of gas and ash proceeding directly from the crater during a paroxysmal eruption. Since no distressing effects were produced, the conclusion is inevitable that gases such as HCl, SO_2, H_2S, CO_2, etc., which are abundant during phases of minor activity, must be practically absent during paroxysmal phases. Since 1907 the normal activity of Stromboli has undergone a radical change which has deprived the volcano of its former title, "The Lighthouse of the Mediterranean." Indeed, so seriously is this the case that a lighthouse is to be erected on Strombolicchio, a monolith of lava which rises from the sea near Stromboli.

IV.—AN INTRODUCTION TO THE GEOLOGY OF NEW SOUTH WALES. By C. A. SÜSSMILCH, F.G.S. 8vo; pp. xviii + 269, with 92 figures and folding coloured map. Sydney: Angus & Robertson, Ltd., 1914. Price 7*s.* 6*d.* net.

TO anyone living in New South Wales, this introduction to its geology should be invaluable. Clearly written, excellently illustrated, and of convenient size, it forms a handbook that every New South Wales geologist should possess. It is not, however, lacking in points of general interest to the casual reader outside the State. To mention only a few of these points—the occurrence of glacial conditions in the Cambrian (p. 17) and their recurrence in the

Permo-Carboniferous (p. 144) and in the Pleistocene (p. 218) of this area, raises questions of more than local interest. The account of available coal in the State is cheering in these days of rapid fuel-consumption. It is estimated that at the present rate of production the available supply will last for another 12,000 years (p. 139). The origin of the artesian water (p. 171); the Cretaceous fossils preserved in opal at White Cliffs (p. 183); the identification of the Tertiary flora with the present day coastal 'brush' flora of very limited range and requiring a warmer, moister climate than that which most of the State now enjoys (p. 203); the post-Tertiary movements which brought about this change in the flora (pp. 212–15); the correlated dwindling in size of the vertebrates from the Tertiary Period onwards (p. 205); the limited area of the Pleistocene glaciation (p. 219); all are examples of interesting points.

The text appears to be singularly free from misprints, but 'spilite' and 'spillite' both occur on p. 61; and 'Queesland' on p. 174 is another slip; while the sentence on p. 181 beginning 'At some localities' lacks construction. 'Outcrop' is frequently used as a verb, and 'intrude' as a transitive verb, neither of which usages, however convenient, is commendable. 'Jasperoid' (p. 62) and 'granitoid' (p. 254) for 'jasper-like' and 'granite-like', 'subsidence area' (p. 152) for 'area of subsidence', and 'Lower' and 'Upper Marine Sea' for 'sea in Lower' and 'sea in Upper Marine times' (legends to figs. 37 and 51) are blemishes; while the equal of 'Trach'te bo'ld'r horiz'n' on p. 129 will only be found in railway time-tables, grocery lists, and printed matter of a similar standing.

Turning to technicalities, we think the statements "The Polyzoa were more abundant than they had ever been before" (p. 133) and "The Gasteropoda . . . were larger than they had ever been before" (p. 134) too sweeping, and strictly inaccurate for a scientific work; "the following genera . . . *Neuroptera*" (p. 135), and "Coralline limestones" (p. 75, presumably for "limestones containing Corals"—not Corallines, an old term for Polyzoa), are inaccuracies. 'Seed-spores' (p. 142) is an extraordinary word. It does not seem in accordance with modern ideas to combine Polyzoa with Brachiopoda in a single phylum Molluscoidea; and the genus *Ammonites* (p. 184) is rather out of date. However abundant *Heliophyllum* may be in Australian Silurian strata, it is best known as a Devonian not a "typical Silurian" Coral (p. 64). Such points might easily be readjusted in a later edition, and have not much weight against the excellencies of this volume.

The illustrations add considerably to the value and interest of the book, and deserve high praise, especially the geological views, of which fig. 44 is a particularly beautiful example. Fig. 47 (of columnar basalt) is very clear; but, while a figure or two is always useful in a geological photograph, the population of fig. 46 is such as to distract the mind from the geological features illustrated. The sections are clearly drawn, and of these fig. 52, showing trough-faults in the Upper Coal-measures, is especially striking. In the figures of fossils the magnification should be given, and it would be useful if the source of the figure, where not original, were noted.

There are several maps showing the distribution of land and water in New South Wales at particular geological periods, and the frontispiece is a clearly-printed, general, geological map of the State. Finally, a glossary (a merciful provision when such words as 'regolith' and 'monadnock' are used) and an index complete this useful handbook.

V.—Brief Notices.

1. The Production of Graphite in 1913. By E. S. Bastin. Mineral Resources of the United States, year 1913. Part II. pp. 181–251. 1914.

THIS report contains all the information on graphite which has appeared in earlier reports of the United States Geological Survey, amplified and brought up to date wherever possible. It includes a valuable account of the physical and chemical properties of graphite, and of the origin and uses of the mineral. The deposits of the United States are described in detail, and descriptions are also given of graphite deposits in Ceylon, Korea, Madagascar, and Mexico. The report is completed by a full bibliography of literature bearing on the occurrence, production, properties, and uses of graphite.

2. Useful Minerals of the United States. Compiled by Samuel Sanford and Ralph W. Stone. United States Geological Survey, Bulletin 585. pp. 250. Washington, 1914.

Lists of useful minerals appeared in the issues of the *Mineral Resources of the United States* for the years 1882 and 1887, but have not been published since. In the interval the mining industry of the United States has increased enormously, and a revised list has long been called for. In view of its length it is now issued as a separate publication. It gives the locality of the principal deposits of useful minerals in the various States, and a glossary showing the composition and character of each mineral and its principal occurrences is added.

3. Summary Report of the Geological Survey, Department of Mines, for the Calendar Year 1913. pp. ix + 544. Ottawa, 1914. Price 20 cents.

This report, which is unusually belated even for an official return, testifies to the extent and varied nature of the work undertaken by the Survey. During the year it was exceptionally heavy owing to the additional field work called for in connexion with the handbooks compiled for the visit of the International Geological Congress the following year (1913). Field work in Canada appears to be not without danger; one of the staff, Dr. J. D. Trueman, unfortunately lost his life owing to a canoe accident, and a topographer was laid up in hospital for some months as the result of an encounter with a grizzly bear. Some progress is reported in fitting up the Natural History Museum, but is greatly handicapped by the lack of properly equipped workrooms and storerooms. Mr. D. D. Cairnes completed the geological section along the 141st parallel between the Yukon and Porcupine Rivers, which is part of the geological section across the

Northern Cordillera undertaken in co-operation with the United States, and found that the formations are dominantly of sedimentary origin, and range from Recent to probably pre-Cambrian age.

4. The Ore Deposits of North-Eastern Washington. By Howland Bancroft, including a section on The Republic Mining District by Waldemar Lindgren and Howland Bancroft. United States Geological Survey, Bulletin 550. pp. 215, with 26 figures and 19 plates. Washington, 1914.

The district described by the author comprises mainly the whole of Stevens and Ferry Counties. The geology could not be determined with certainty owing to the absence of fossils, but the rocks may be referred to the Proterozoic, Palæozoic, Mesozoic, and Cenozoic eras. One of the most conspicuous rocks is an intrusive granite. The mineral resources are very varied, and include gold, silver, lead-zinc, copper, iron, tungsten, nickel, antimony, and molybdenite deposits, and also minerals used as fluxes. The various mines are described in detail, and reference to the memoir is facilitated by an excellent index.

5. Electric Activity in Ore Deposits. By Roger C. Wells. United States Geological Survey, Bulletin 548. pp. 78. Washington, 1914.

As the result of considerable investigation in the laboratory the author concludes that electric action may have played no small part in the deposition of ores. Many metalliferous minerals can conduct electricity and act as electrodes and as conductors of electric currents in ore deposits. The chemical difference producing the greatest effect appears to be that existing between oxidizing and reducing solutions. Pyrites is so inert to many solutions as to function electrically like unattackable electrodes for long periods, thus making oxidizing or reducing solutions available for producing electric currents in ore deposits.

REPORTS AND PROCEEDINGS.

I.—Geological Society of London.

February 3, 1915.—Dr. A. Smith Woodward, F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Gravels of East Anglia." By Professor T. McKenny Hughes, M.A., F.R.S., F.G.S.

The author discusses the sources from which the subangular gravels that cover such large areas in East Anglia can have been derived.

He points out that their great variety of fracture, colour, etc., proves that they cannot have come directly from the Chalk, nor from Boulder-clay derived directly from the Chalk, nor from the Lower London Tertiaries, none of which contain subangular gravels, but only beds of pebbles, and those mostly of small size.

The character of the flints in the gravels indicates that they have been derived from surface-soils which have been winnowed and shifted by soil-creep, rain, and streams, until arrested on the terraces and flats of the valleys.

The dry land of Miocene age was the first over which the flints of our gravel beds could have received that subaerial treatment which they all seem to have undergone. There was then no Boulder-clay to protect and obscure the Chalk-with-Flints.

Then came the submergence which let in the Crag sea. This rapidly invaded the land, not giving time to reduce the flints to pebbles, but burying remains of animals and plants in coarse subangular gravel. In time the subsidence affected more distant shores, and compensating rises of mountain regions far away began to modify climatal conditions; ice floated southwards, stranding at various depths according to size, ploughed up and crumpled the shore-deposits, and dropped masses of far-transported material. It is not difficult to distinguish the old shore-deposits, even when they have been crumpled up, from the foreign material introduced by the floating ice.

When the land had sunk so low that the wind- and tide-waves could not sort the material, it remained, as brought, a boulder-clay, which is therefore widely spread over the peneplain of the East Anglian heights, and is generally above the gravel and sand of the advancing sea.

Where the sea was able to work longer at pounding and rolling the flints, immense beds of pebbly shingle or of sand are the result.

The Plateau Gravel is traced from section to section across the country, and the characteristics by which it can be recognized are pointed out.

Since an irregular land-surface was thus depressed beneath the sea, one might expect that in some of the deeper valleys deposits older than the submergence might be detected. Also, seeing that the land has slowly risen again many hundred feet, we ought to have evidence of the subaerial denudation which has been going on since the land began to rise.

Thus the author starts with the definition of three important ages of long duration, and proceeds to refer some of the best exposed gravel deposits of East Anglia to one or other of them.

They are, in descending order—

(1) The stage of which the Barnwell Gravel is taken as a type.
(2) The stage of which the Plateau Gravel is the most important representative.
(3) The stage to which he suggests that the Barrington Beds may belong.

The rest of the paper consists of descriptions of sections and discussion of evidence derived from fossil remains.

2. "The Pitchstones of Mull and their Genesis." By Ernest Masson Anderson, B.Sc., M.A., F.G.S., and E. G. Radley.

The pitchstones here discussed occur with extraordinary frequency, intruded into the Tertiary plateau-lavas of the eastern portion of the Ross of Mull, as well as in less number in other parts of the island.

They fall into two main divisions, distinguished by the absence or by the presence of porphyritic felspars. Those of the non-porphyritic class are the most prevalent, and usually form the central portion of sills or inclined sheets. The marginal portion of these intrusions is crystalline or stony. The petrological characters of these pitchstones and their more crystalline margins are such that they seem to warrant the grouping of the rocks under a new type-name, and the name leidleite has been chosen. The porphyritic pitchstones occur as flat or gently inclined sheets; they also are associated with a more crystalline phase, and have been grouped under the type-name inninmorite.

The relation of the stony margins of the pitchstone intrusions to their glassy centres is usually seen clearly. A typical leidleite may have 5 feet of pitchstone in the centre, with margins of stony matter 3 feet thick on each side. Occasionally the central glassy portion may be split up by stony partings.

A feature that occurs very frequently in these rocks is what has been termed 'sheath and core' structure. In this case the stony base and top of an intrusion send off narrow sheets of stony character which traverse the glassy portion in a branching and sinuous manner. The glassy nature of the cores is clearly not due to a greater rapidity of cooling; but, with the object of ascertaining the reason for the devitrification, a chemical investigation of both the glassy and stony portions was undertaken.

It has been found that there is a much greater percentage of water given off from the rock at 105° C. in the case of the glassy variety, and the authors suggest that the escape of this excess of water, soon after the consolidation of the rock, has resulted in the devitrification of the sheaths and margins.

II.—Mineralogical Society.

January 26, 1915.—Dr. A. E. H. Tutton, F.R.S., President, in the Chair.

S. Kôzu: The Dispersion of Felspar. By the most refined methods the dispersions in the three principal directions were determined for various members of the felspar group.—F. P. Mennell: Note on the Colour of some Alluvial Diamonds and of Pyrrhotite. The colour, usually green, of the diamonds in the gravels of Somabula, Rhodesia, is superficial, and probably due to infiltration, presumably of iron salts, while the stones were lying where they now occur. Pyrrhotite is tin-white in colour when fresh. The cause of its rapid alteration was discussed.—Professor G. Cesàro: Crystals of Calomel from Spain. The crystals, which were pale-yellowish in colour, imperfectly transparent, from 1 to 3 mm. in size, and displaying the forms 100, 111, 311, were most irregularly developed.—Professor G. Cesàro: General Formula for the Birefringence of a Crystal-plate in terms of the Angles which its Normal makes with the Principal Optical Axes. The approximate formula is obtained by supposing the mean index of refraction to become infinite, while the differences between it and the greatest and least indices remain constant.—

Professor G. Cesàro: On a Numerical Relation of the Sum of the Symmetry-axes situated in the Symmetry-planes of a Polyhedron. If $N \cdot A_n$, $P \cdot A_p$, $Q \cdot A_q \ldots$ are the axes of symmetry lying in the planes of symmetry, then $4\{Nn(n-1)+Pp(p-1)+Qq(q-1)+\ldots\}+1=C$, and the number of planes of symmetry is given by $X=\frac{C+1}{2}$

OBITUARY.

ARTHUR ROOPE HUNT, M.A. (CANTAB.), F.L.S., F.G.S.

BORN JANUARY 8, 1843. DIED DECEMBER 19, 1914.

WITH the close of the past year another of our old friends has been taken from us, and one who for a quarter of a century was not only a friend, but a frequent contributor to the pages of the GEOLOGICAL MAGAZINE.

Arthur Roope Hunt was descended from an old Devonshire family, who had resided for generations in or near Dartmouth. He was the son of Mr. Arthur Hunt, partner in the firm of Messrs. Hunt, Roope, & Teage, wine exporters of Oporto, and there, in 1843, young Hunt was born. But his residence in Portugal was only of brief duration. When only 8 or 9 years of age he left with his parents hurriedly in an English war vessel, as the lives of the British residents in Oporto were endangered by a revolution.

His family settled in Torquay in 1852. Here he commenced his English education under the tuition of the Rev. Townsend Warner, whose pupils included the present Lord Rayleigh, one of Hunt's school, college, and lifelong friends. Another youthful companion, only two years his senior, was afterwards to become Field-Marshal Lord Grenfell.

Those early years must have been very happy ones, for although always a delicate lad, Arthur Hunt enjoyed abundant outdoor pleasures and had many friends willing to share his society and encourage his pursuits.

It must be borne in mind that Torquay had been, from an early date, a very active centre of scientific life in all its diversified branches. The well-known Torquay Natural History Society, which was founded by William Pengelly and others, now in its seventieth year, afforded an admirable focus to a very wide circle of men of leisure and education resident in South Devon. In addition to Mr. Pengelly, Hunt's earliest instructor in geology, may be mentioned the naturalist, Mr. Philip Henry Gosse, the Rev. T. R. R. Stebbing, the Rev. G. F. Whidborne, Mr. John Edward Lee, Mr. E. B. Tawney, Mr. Daniel Pidgeon, Mr. R. H. Worth, and in later years Mr. Arthur Champernowne, Mr. W. A. E. Ussher, Mr. A. J. Jukes-Browne, and Mr. Alexander Somervail, to all of whom Hunt was intimately known.

Mr. Hunt displayed much knowledge of engineering, and had his constitution been more robust he might have carried on much more elaborate investigations; but his health forbade it, and he devoted himself to open-air pursuits, chiefly to geology, marine physics, and zoology.

Mr. Edmund Gosse, writing of his father, Philip Henry Gosse (the well-known author of many works on marine zoology), says: "When he [Gosse] took sailing excursions he often had the company of Mr. Arthur Hunt of Torquay, a young naturalist of knowledge and enthusiasm who possessed a yacht, the *Gannet*, in which the friends undertook frequent scientific excursions, especially over the sandy *zostera*-beds in Torbay, among the little archipelago which lies off Hope's Nose, at the mouth of Brixham Harbour, and off Berry Head."

At 18 A. R. Hunt proceeded to Trinity College, Cambridge, where in 1864 he took his degree of M.A. He then studied law, "ate his dinners" at the Inner Temple, and was duly "called to the Bar", but he never practised.

After spending a few years in the business house of a cousin in the city of London, he abandoned town permanently, and in 1874 settled down at Southwood, Torquay, only varying his residence to visit his estate at Foxworthy, Moreton Hampstead.

In 1870 he was elected F.G.S., and in 1884 he became a Fellow of the Linnean Society.

In company with Mr. Pengelly he devoted much time to the exploration of Kent's Cavern, and wrote many papers thereon. What he learned from Pengelly he applied to the Scottish cave at Borness, Kirkcudbrightshire, which he explored with the co-operation of Adam Corrie & W. Bruce-Clarke,[1] a very complete and excellent piece of cave-work.

Accompanied by Pengelly, Tawney, and other of his friends, he made numerous geological investigations in Devonshire and neighbouring counties. He also specially studied the subject of ripple-mark and its origin and the submarine geology of the English Channel off the coast of Devon and Cornwall. Mr. Hunt secured numerous rock-specimens brought in by the Brixham trawlers, many of which were sliced and examined microscopically and reported upon by him. With Mr. R. N. Worth he devoted special attention to the age of the Dartmoor granites, the Devonian rocks of South Devon, and the metamorphic schists and Lizard serpentines, and published numerous papers thereon. Over these subjects he was both the giver and the recipient of much keen criticism from General MacMahon, Professor Bonney, and others. Many of these controversies were carried on in the pages of the Geological Magazine, the Transactions of the Devonshire Association (1892–7), and other journals.

For many years he attended the meetings of the British Association, and he took a keen interest in the papers read in the various sections, and joined in the discussions with his usual enthusiasm.

Like other able young men of science Hunt felt attracted by and tempted to cope with questions relating to many and diverse branches of research, and delighted, as the Athenians of old, "to tell or to hear of some new thing." Had he devoted himself exclusively to any one of the varied subjects he at times pursued with so much ardour, he might have earned a more distinguished name for himself in science outside Devonshire, where, and especially in Torquay, he will be long

[1] See Proc. Soc. Antiq. Scotland, vol. x, 1873–4, illustrated by six octavo plates prepared from photographs by A. R. Hunt.

remembered. The stimulus of *necessity* was, however, wanting, and Mr. Hunt could allow himself the pleasant freedom of the amateur to take up and lay aside a number of diverse pursuits, to turn from the microscope and the study of igneous rocks to pen a long letter to the local or provincial press, voicing his views on the various actions of the Municipal Authorities and the needs of Torquay and its harbour, and on the hundred and one other matters a Corporation undertakes.

In early years I paid many pleasant visits to my old friend Mr. John Edward Lee, antiquary and geologist, at Torquay. There I met Arthur Roope Hunt, and with him I studied ripple-mark, and in his boat we visited the raised beaches on the Thatcher. He also showed me his model arrangement for demonstrating the force and velocity of waves and their action on the stability of lighthouses. He was full of enthusiasm and interest, and I look back to my friendship with him as a most pleasant memory. The loss of such a versatile man of genius will be much felt by the wide circle in Devonshire and elsewhere with whom he came in contact, either personally or by correspondence, for he was a great letter-writer.

Between 1890 and 1913 Mr. Hunt frequently contributed to the Geological Magazine, and to the Transactions of the Devonshire Association from 1873 to 1913. His paper on "Ripple-mark" was read by Lord Rayleigh before the Royal Society in 1882 (see Proc. Roy. Soc.). Other products of his pen appeared in the publications of the Torquay Natural History Society, the Proceedings of the Royal Dublin Society, the Linnean Society, the British Association, the Society of Antiquaries of Scotland, and in the *Westminster Review*. He published altogether nearly a hundred papers, whilst his letters on scientific and general topics in the *Torquay Directory* and other newspapers probably reached several hundred in number. He was a past President of the Torquay Natural History Society, one of the founders and managers of its Museum, and its frequent benefactor. On no less than three occasions he felt compelled to decline the proffered honour of the presidency of the Devonshire Association.

In his yachting days he was a member of the Royal Dart Yacht Club; he was a former Captain of the Torquay Golf Club; and Captain of the Miniature Rifle Club at Walls Hill. Amongst his varied attainments he was an enthusiastic musician, and an accomplished photographer.

Mr. Hunt leaves a widow and a son, Mr. C. A. Hunt, M.A., Barrister-at-Law, and one married daughter, Mrs. Ernest Smith.

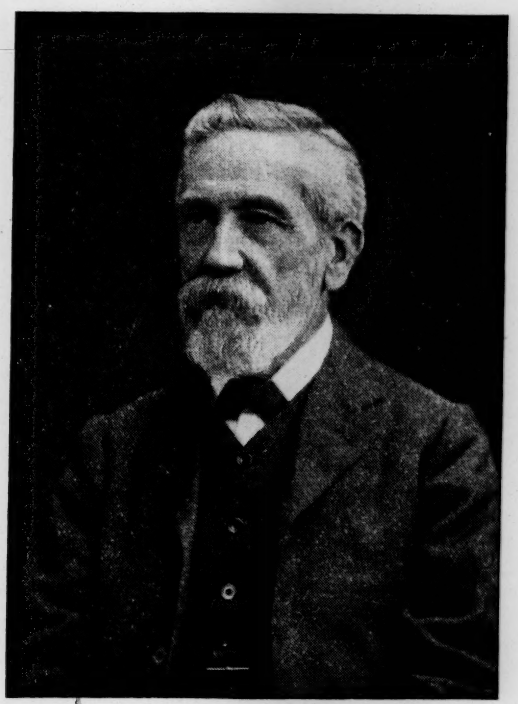
H. W.

FREDERICK WILLIAM RUDLER, I.S.O., F.G.S., Etc.

Born July, 1840. Died January 23, 1915.

The death of Mr. F. W. Rudler took place at Tatsfield, Surrey, on January 23, and will cause the deepest sorrow to a very wide circle of geological and other scientific friends, by whom he was highly esteemed for his wide literary and scientific attainments, and beloved for his gentle and kindly disposition. His modesty almost amounted to shyness. He was a friend to those in trouble and a generous helper to those in need. Some fifty-five years have passed away

since Mr. Rudler began his scientific career as a student at the Regent Street Polytechnic Science and Art Classes, where his remarkable talents resulted in his being awarded two gold medals (the highest award) in one year; and this doubtless led to his appointment, in 1861, to the post of Assistant Curator in the Museum of Practical Geology in Jermyn Street. In 1876 he was appointed lecturer in Natural Science in the University College of Wales at Aberystwyth, where he remained for three years and, in addition to



F. W. Rudler.
1910.

his lectures, established the Museum of the College, afterwards unfortunately destroyed by fire. In 1879, on the death of Mr. Trenham Reeks, Mr. Rudler was recalled to Jermyn Street Museum at the urgent request of the Director, Sir Andrew Ramsay, where he undertook the duties of Registrar of the Royal School of Mines and Curator and Librarian of the Museum of Practical Geology. The former post Mr. Rudler held until the final removal of the School of Mines to South Kensington, and the latter until his retirement in

1902. His duties during these twenty-two years were strenuous and trying owing to the great and critical changes in the establishment which took place, changes much deprecated by our old friend, who was dearly attached to the Survey and its Museum. The most obvious of these changes was the final removal of the Royal School of Mines and the division of the Library, nearly half the books being carried off to South Kensington, and the sad depletion of the Museum by the elimination of the unique collections of pottery and of metal-work.

Mr. Rudler served under four well-known Directors, Sir Roderic Murchison, Sir Andrew Ramsay, Sir Archibald Geikie, and Dr. J. J. H. Teall, and so much were his services appreciated by them and by the Science and Art Department at South Kensington that, upon his retirement, he received from King Edward the Imperial Service Order.

As a mineralogist Mr. Rudler was most accomplished, being able to identify and name any mineral specimen at sight, and could state its properties and locality with wonderful precision.

Beyond his official duties Mr. Rudler was also an eloquent speaker and a voluminous writer. He was intimately connected with a number of our scientific societies, and for many years took a prominent part in the British Association meetings. His long courses of lectures for the Society for the Extension of University Teaching were highly appreciated, and he was constantly called upon to lecture upon special subjects in which he greatly excelled. Much of his writing was devoted to works on technical science; the 1875 edition of Ure's *Dictionary of Arts and Manufactures* was chiefly, one may say, his writing. Many articles of his will be found in the *Encyclopædia Britannica*, in Thorpe's *Dictionary of Applied Chemistry*, in Muir's *Dictionary of Chemistry*, and elsewhere. Nor must one omit to mention the *Guide to the Museum of Practical Geology* and his *Catalogue of Pottery and Porcelain*. His scientific reviews are to be found scattered through some of our leading journals for years past, and his connexion with the *Athenæum* was continued until his death.

Mr. Rudler was elected a Fellow of the Geological Society in 1870, and was awarded the Lyell Medal by the Council in 1903, in recognition of his great services to geological science by his lectures and his writings. He joined the Geologists' Association in 1874, and was elected President in 1887–9. Special reference must be made to his Presidential Address, "Fifty Years Progress in British Geology," and to his masterly essays on Experimental Geology.

On the occasion of the Fourth Session of the International Geological Congress, held in London September 17–23, 1888, under the Presidency of Professor Prestwich, F.R.S., Mr. F. W. Rudler was appointed Honorary Treasurer, and fulfilled the difficult task to the great advantage of the members of the Congress, from whom he received grateful thanks.

Some few years ago our dear friend, finding it necessary to give up very much of his lecturing and literary labours, retired to his quiet home at Tatsfield, where he resided until he passed away peacefully to his rest.

E. T. N.